ERIC Document Resume	See Instruction Sheet	DATE OF RESUME	DATE RECEIVED

A. PUBLICATION DATA

1. ERIC NUMBER	2. ORIGINATOR CONTROL NO(S).	3. IDENTIFICATION NO.
ED001707	N.A.	CR 727

DO NOT WRITE IN SPACE BELOW

4. AUTHOR(S):
Suppes, Patrick

5. TITLE OF DOCUMENT ABSTRACTED:
The Development of Mathematical Concepts in Children

6. PUBLICATION TITLE:
N.A.

7. EDITOR(S):
N.A.

9. PUBLISHER OR SOURCE Cooperative Research Program, Office of Education Department of HEW, Washington, D.C.

10. VOL.	11. PAGES	12. MO./YR.	13. TOTAL PP.
N.A.	N.A.	1964	50

8. PLACE OF ORIGIN
ORG. Stanford University
ST. Stanford, California
CITY, STATE
TELE. EXT.
INST/LOC CODE

MICROFILM COPY AVAILABLE
YES ☒ NO ☐

COPYRIGHT GRANTED
YES ☐ NO ☒

COPYRIGHT RELEASE FILED
YES ☐ NO ☐

TYPE OF REPORT
PROPOSAL ☐
IN-PROGRESS ☐
FINAL ☒
FOLLOW-UP ☐

OTHER ERIC REFERENCES:

ERIC SATELLITE CODE

B. INDEXING DATA

14. SUBJECT AREA

15. TYPE OF ENDEAVOR
report

16. INDEXING TERMS (6-12)

math concepts	teaching methods	learning, incidental		
curriculum	binary numbers	mathematics		
work-study	set theory			

17. ABSTRACT (MAX. 250 WORDS)

The role of rote memorization with regard to the perception of mathematical concepts was investigated. The concepts included for experimentation were: binary numbers, sets, polygons and angles, stimulus variation, display, incidental learning, and response variation. It was concluded that the formation of simple concepts in young children is an all-or-none process. Learning is more efficient if the child who makes an error is required to make an overt correction response in the presence of the stimulus to be learned. Incidental learning does not appear to be an effective method of acquisition. A pilot study on math proofs is described.

ERIC No.

18. SOURCE OF ABSTRACT

C. FUNDING DATA

19. SUPPORTING ORGANIZATION

20. DIVISION OF DEPT.

21. CONTRACT NO.

PROJECT NO.

22. TOTAL COST OF PROJECT

23. DURATION OR PROJECT

U.S. Department of Health, Education, and Welfare
Office of Education

REPRINTED BY PERMISSION
U. S. DEPARTMENT OF HEALTH, EDUCATION AND WELFARE
OFFICE OF EDUCATION

THIS DOCUMENT HAS BEEN PRINTED EXACTLY AS RECEIVED FROM THE PERSON OR ORGANIZATION ORIGINATING IT. POINTS OF VIEW OR OPINIONS STATED DO NOT NECESSARILY REPRESENT OFFICIAL OFFICE OF EDUCATION POSITION OR POLICY.

ED001707

THE DEVELOPMENT OF MATHEMATICAL CONCEPTS IN CHILDREN

COOPERATIVE RESEARCH PROJECT No. 727

PATRICK SUPPES

STANFORD UNIVERSITY
STANFORD, CALIFORNIA

THE DEVELOPMENT OF MATHEMATICAL CONCEPTS IN CHILDREN

Cooperative Research Project No. OE 727

Final Report

Patrick Suppes

Stanford University
Stanford, California

1964

The research reported herein was supported by the Cooperative Research Program of the Office of Education, U. S. Department of Health, Education, and Welfare

The Development of Mathematical Concepts in Children[1)]

Patrick Suppes

1. Introduction

The aims of this report are primarily constructive, that is, to contribute to the development of a scientific theory of concept formation. However, before I turn to this subject, there are two general aspects of the teaching of mathematical concepts I want to comment upon.

1/ This report in a slightly revised form and under the title "On the behavioral foundations of mathematical concepts" will be published as part of the proceedings of a conference on mathematics learning held in Berkeley on May 4-6, 1962, and sponsored jointly by the Social Science Research Council and the Carnegie Corporation of New York.

The first concerns the much heard remark that the newer revisions of the mathematics curriculum are particularly significant because of the emphasis they are placing on understanding concepts as opposed to the perfection of rote skills. My point is not to disagree with this remark, but to urge that it is essentially banal. It is a good thing to understand; it is a bad thing to possess mere rote skill. The banality arises from not knowing what we mean by understanding. This failure is not due to disagreement over whether the test of understanding should be a behavioral one. I am inclined to think that most people concerned with this matter would admit the central relevance of overt behavior as a measure of understanding. The difficulty is rather that no one seems to be very clear about the exact specification of the behavior required to exhibit understanding. Moreover, apart even from any behavioral questions the very notion of understanding seems fundamentally vague and ill-defined.

To illustrate what I mean, let us suppose that we can talk about understanding in some general way. Consider now the concept of triangularity. Does understanding this concept entail understanding that the sum of the interior angles is 180°, or that triangles are rigid whereas quadrilaterals are not, or being able to prove that if the bisectors of two angles of a triangle are equal then the triangle is isosceles? This example suggests one classical philosophical response to our query, namely, to understand a concept means, it is said, to know or believe as true a certain set of propositions that use the concept. Unfortunately, this set is badly defined. It is trivial to remark that along these lines we might work out a comparative notion of

understanding that is a partial ordering defined in terms of the inclusion relation among sets of propositions that use the concept. Thus one person understands the concept of triangularity better than a second if the set of propositions that use the concept and that are known to the first person includes the corresponding set for the second person. (Notice that it will not do to say simply that the first person knows more propositions using the concept, for the second person might know fewer propositions but among these would be some of the more profound propositions not known by the first person; this situation corresponds to the widely held and probably correct belief that the deepest mathematicians are not necessarily the best mathematical scholars.)

But this partial ordering does not take us very far. A more behavioral line of thought that at first glance may seem more promising is the response of the advocates of programmed learning to the charge that the learning of programmed material facilitates rote skills but not genuine understanding of concepts. The response is to assert that if the critics will simply specify the behavior they regard as providing evidence of understanding, they, the programmers, will guarantee to develop and perfect the appropriate repertoire of responses. This approach has the practical virtue of sidestepping any complex discussion of understanding, and supposes, with considerable correctness no doubt, that without giving an intellectually exact analysis of what it means to understand a concept, we still can obtain a rough consensus at any given time of what body of propositions we expect students to master about a given concept. This is the appropriate practical engineering approach, but it scarcely touches the scientific problem.

In this paper I do not pretend to offer any serious characterization of what it means to understand a concept. I do think that the most promising direction is to develop a psychological theory of concept transfer and generalization. The still relatively primitive state of the theory of the much simpler phenomena of stimulus transfer and generalization do not make me optimistic about the immediate future. For immediate purposes, however, let me sketch in a very rough way how the application of ideas of transfer and generalization can be used to attack the banality mentioned earlier in the standard dichotomy of understanding versus rote skill. We would all agree, I think, that such matters as learning to give the multiplication tables quickly and with accuracy are indeed rote skills. But there is also what I consider to be a mistaken tendency to extend the label "rote skill" to many parts of the traditional mathematics curriculum at all levels. The body of mathematical material tested, for example, by the British Sixth Form examinations is sometimes so labeled by advocates of the newer mathematics curriculum. In terms of the accepted notion of rote skill developed and studied by psychologists this is a mistake, for the production of a correct response on these examinations cannot be explained by any simple principles of stimulus-response association. Moreover, the problems of transfer involved in solving typical British Sixth Form examination problems in comparison with the kind of examination set by advocates of the newer mathematics curriculum may in fact require more transfer of concepts. At least more transfer in one obvious way of measuring transfer, that is, in terms of the number of hours of training spent in relation to the ability to solve the problems on the

part of students matched as to general background and ability. I recognize that these are complicated matters and I do not want to pursue them here. Also I am fully in sympathy with the general objectives of the newer mathematics curriculum. I am simply protesting here against some of the remarks about understanding and rote skills that occur in the pedagogical conversations and writings of mathematicians.

The second general point I want to mention briefly is of a similar sort. I have in mind the many current discussions of the efficacy of the discovery method of teaching. The discussions of this method seem to provide yet one more remarkable example in the history of education of a viewpoint coming to prominence without any serious body of empirical evidence to support or refute its advocates. From the standpoint of learning theory I do not even know of a relatively systematic definition of the discovery method. I do not doubt that some of the advocates of this method are themselves remarkably capable teachers and are able to do unusual and startling things with classes of elementary school children. The intellectual problem, however, is to separate the pedagogical virtuosities of personality of these advocates from the systematic problem of analyzing the method itself. Workable hypotheses need to be formulated and tested. I know that it is a standard objection of some advocates of the discovery method that any quick laboratory examination of this teaching method versus a more standard immediate reinforcement method, particularly as applied to young children, is bound not to yield an unbiased test. The results and the implications of the methods, it is said, can only be properly evaluated after a long period. I personally rather doubt that this is the case but, if it is so, or if

it is propounded as a working hypothesis by advocates of the method, then it seems to me that it is their intellectual responsibility to formulate proper tests of a sufficiently sustained sort. I realize that my remarks on this subject in this paper have the character of obiter dicta. On the other hand, in a more complete treatment of mathematical concept formation in young children, I would consider it necessary to probe more deeply into the issues of motivation, reinforcement and concept formation surrounding the controversy between the discovery method and other more classical methods of reinforcement. Some experimental results on methods of immediate reinforcement are reported in Section 3.

I turn now to the specific topics I would like to develop more systematically. In the next section a version of stimulus sampling learning theory is formulated that holds considerable promise for providing a detailed analysis of the behavioral processes involved in the formation of mathematical concepts. In the following section I report in somewhat abbreviated form six experiments dealing with mathematical concept formation in young children. A particular emphasis is placed on whether the learning process in this context is represented better by all-or-none or incremental conditioning. Section 4 is concerned with behavioral aspects of logical inference, and in particular, of mathematical proofs.

2. Fundamental Theory.

The fundamental theory I shall apply in later sections is a variant of stimulus sampling theory as first formulated by Estes [1950]. The axioms given here are very similar to those found in Suppes and Atkinson [1960]. I shall not discuss the significance of the individual axioms at length because this has been done in print by a number of people. The axioms I may mention, however, are based on the following postulated sequence of events occurring on a given trial of an experiment. The organism begins the trial in a certain state of conditioning. Among the stimuli available a certain set is sampled. On the basis of this sampled stimuli and their conditioning connections to the possible responses a response is made. After the response is made, a reinforcement occurs which may change the conditioning of the sampled stimuli. The organism then enters a new state of conditioning ready for the next trial. The following axioms (divided into conditioning, sampling and response axioms) attempt to make the assumptions underlying such a process precise. They are given in verbal form but it is a routine matter to translate them into an exact mathematical formulation.

Conditioning Axioms

C1. On every trial each stimulus element is conditioned to at most one response.

C2. If a stimulus element is sampled on a trial, it becomes conditioned with probability c to the response (if any) that is reinforced on that trial; if it is already conditioned to that response, it remains so.

C3. If no reinforcement occurs on a trial, there is no change in conditioning on that trial.

C4. Stimulus elements that are not sampled on a given trial do not change their conditioning on that trial.

C5. The probability c that a sampled stimulus element will be conditioned to a reinforced response is independent of the trial number and the preceding pattern of events.

Sampling Axioms

S1. Exactly one stimulus element is sampled on each trial.

S2. Given the set of stimulus elements available for sampling on a trial, the probability of sampling a given element is independent of the trial number and the preceding pattern of events.

Response Axioms

R1. If the sampled stimulus element is conditioned to a response, then that response is made.

R2. If the sampled stimulus element is unconditioned, then there is a probability p_i that response i will occur.

R3. The guessing probability p_i of response i when the sampled stimulus element is not conditioned is independent of the trial number and the preceding pattern of events.

Although it is not stated in the axioms, it is assumed that there is a fixed number of responses and reinforcements and a fixed set of stimulus elements for any specific experimental situation.

Axioms C5, S2 and R3 are often not explicitly formulated by learning theorists but for the strict derivation of quantitative results they

are necessary to guarantee the appropriate Markov character of the sequence of state-of-conditioning random variables. Axioms of this character are often called independence-of-path assumptions.

The theory formulated by these axioms would be more general if Axiom S1 were replaced by the postulate that a fixed number of stimuli are sampled on each trial or that stimuli are sampled with independent probabilities, and Axiom R1 were changed to read that the probability of response is the proportion of sampled stimulus elements conditioned to that response, granted that some conditioned elements are sampled. For the experiments to be discussed in the next section this is not an important generalization and will not be pursued here. (From the historical standpoint the generalizations just mentioned actually were essentially the original ones of Estes.) Nowadays they are referred to as the assumptions of the component model of stimulus sampling. Axiom S1 as formulated here is said to formulate the pattern model, and the interpretation is that the organism is sampling on a given trial the pattern of the entire stimulating situation, at least the relevant pattern, so to speak. This pattern model has turned out to be remarkably effective in providing a relatively good, detailed analysis of a variety of learning experiments ranging from rats in T-mazes to two-person interaction experiments.

There is one other general remark I would like to make before turning to the discussion of particular experiments. The kind of stimulus response theory just formulated is often objected to by psychologists interested in cognitive processes. I do not doubt that empirical objections can be found to stimulus response theory when

stated in too simple a form. However, I am prepared to defend the proposition that at the present time there is no other theory in psychology that can explain in the same kind of quantitative detail an equal variety of learning experiments, including concept formation experiments. I should also add that I do not count as different, cognitive formulations that are formally isomorphic to stimulus sampling theory. In our recent book Atkinson and I [1960] attempted to show how the hypothesis language favored by many people (e.g., Bruner, Goodnow and Austin [1956]) can be formulated in stimulus sampling terms. For example, a strategy in the technical sense corresponds precisely to a state of conditioning and a hypothesis to the conditioned stimulus sampled on a given trial, but it is not to the point here to work out details of this comparison.

3. Some Concept Experiments with Children.

I now turn to the applications of the fundamental theory stated in the preceding section to a number of experiments concerned with concept formation in young children. It would be possible first to describe these experiments without any reference to the theory, but in order to provide a focus for the limited amount of data it is feasible to give in this survey, it will be more expedient to specialize the theory initially to the restricted one-element model, and report on data relevant to the validity of this model.

We obtain the one-element model by extending the axioms given in the preceding section in the following respect. We simply postulate that there is exactly one stimulus element available for sampling on each trial and that at the beginning of the experiment this single element is unconditioned.

This special one-element model has been applied with considerable success by Bower (1961] and others to paired-associate experiments, that is, to experiments in which the subject must learn an arbitrary association established by the experimenter between, say, a nonsense-syllable as single stimulus and a response such as one of the numerals 1-8 or the pressing of one of three keys. The most important psychological implication of this one-element model is that in the paired-associate situation the conditioning takes place on an all-or-none basis. This means that prior to conditioning the organism is simply guessing the correct response with the probability p_i mentioned in Axiom R3, and that the probability of conditioning on each trial in which the stimulus is presented is c. Once the stimulus is conditioned the correct response is made with probability one.

In an earlier paper Rose Ginsberg and I [1963] analyzed a number of experiments, including some of those reported here, to exhibit a simple but fundamentally important fact about this all-or-none conditioning model. The assumptions of the model imply that the sequence of correct and incorrect responses prior to the last error form a binomial distribution of Bernoulli trials with parameter p. This null hypothesis of a fixed binomial distribution of responses prior to the last error admits at once the possibility of applying many powerful classical statistics that are not usually applicable to learning data. What is particularly important from a psychological standpoint is the implication of this hypothesis that the mean learning curve, when estimated over responses prior to the last error, will be a horizontal line. In other words, no effects of learning should be shown prior to conditioning.

Ginsberg and I analyzed experiments concerned with children's concept formation, animal learning, probability learning and paired-associate learning in adults from this standpoint. I shall not propose to give as extensive analysis of data in the present paper as we attempted there but I will attempt to cite some of the results on this question of stationarity because of its fundamental importance for any psychological evaluation of the kind of processes by which young children acquire concepts.

Other features of the experiments summarized below will be mentioned seriatim, particularly if they have some bearing on pedagogical questions. There is, however, one general methodological point that should be mentioned before individual experiments are described. In many of the experiments described below the stimulus displays were different on every trial, so that there was no possibility of establishing a simple stimulus-response association. How is the one-element model to be applied to such data? The answer represents, I think, one of our more important general findings. It is that a very good account of much of the data may be obtained by treating the concept itself as the single element. The schema is then that a simple concept-response association is established. With the single exception of Experiment I, it is with this interpretation that we have applied the one-element model to our experiments. Some further ramifications of this viewpoint are explored in the final section.

Experiment I. Binary Numbers.

This experiment is reported in detail in Suppes and Ginsberg [1962a] Five and six year old subjects were required to learn the concepts of

the numbers 4 and 5 in the binary number system, each concept being represented by three different stimuli; for example, if the stimuli had been chosen from the Roman alphabet, as in fact they were not, 4 could have been represented by abb, cdd, and eff, and 5 by aba, cdc and efe. The child was required to respond by placing directly upon the stimulus one of two cards. On one card was inscribed a large Arabic numeral 4 and on the other a large Arabic numeral 5. All the children were told on each trial whether they made the correct or incorrect response, but half of them were also required to correct their responses following an error. Thus in this experiment in addition to testing the one-element model we were concerned to examine the effect upon learning of requiring the subject to correct overtly a wrong response. There were twenty-four subjects in each of the two experimental groups. From test responses, after each experimental session, it seemed evident that whereas some subjects in both groups learned the concept as such, others learned only some of the specific stimuli representing the concepts, so that in effect within each group there were two sub-groups of subjects. It is interesting to note that this finding is in agreement with some similar results in lower organisms (Hull and Spence [1938]), but contrary to results obtained with adult subjects, for whom an overt correction response seems to have negligible behavioral effects (Burke, Estes and Hellyer [1954]).

The data for both correction and non-correction groups are shown in Fig. 1. It is apparent from the figure that there was a significant

---

Insert Figure 1 about here

---

difference between the two groups in the rate of learning. The t of 4.00 computed between overall responses of the two groups is significant at the .001 level.

For the analysis of paired-associates and concept formation we restricted ourselves to the twenty-four subjects of the correction group. To begin with, we analyzed the data as if each of the six stimuli, three for each number, represented an independent paired-associate item. In Fig. 2 are shown the proportion of correct responses prior to the last error and the mean learning curve over all responses, both presented from this point of view.

Insert Figure 2 about here

The data points are for individual trials. Because a total of only sixteen trials were run on each stimulus we have adopted a criterion of six successive correct responses and thus the proportion of correct responses prior to the last error are shown only for the first ten trials in Fig. 2. A χ^2 test of stationarity over blocks of single trials supports the null hypothesis ($\chi^2 = 8.00$, df = 9, $P > .50$, $N = 844$).

Let us now turn to the question of concept formation. The identification we make has already been indicated. We treat the concept as itself the single stimulus, and in this case we regard the experiment as consisting of two concepts, one for the number 4 and one for the number 5. (It should be apparent that the identification in terms of the numbers 4 and 5 is not necessary; each concept can be viewed simply as an abstract pattern.)

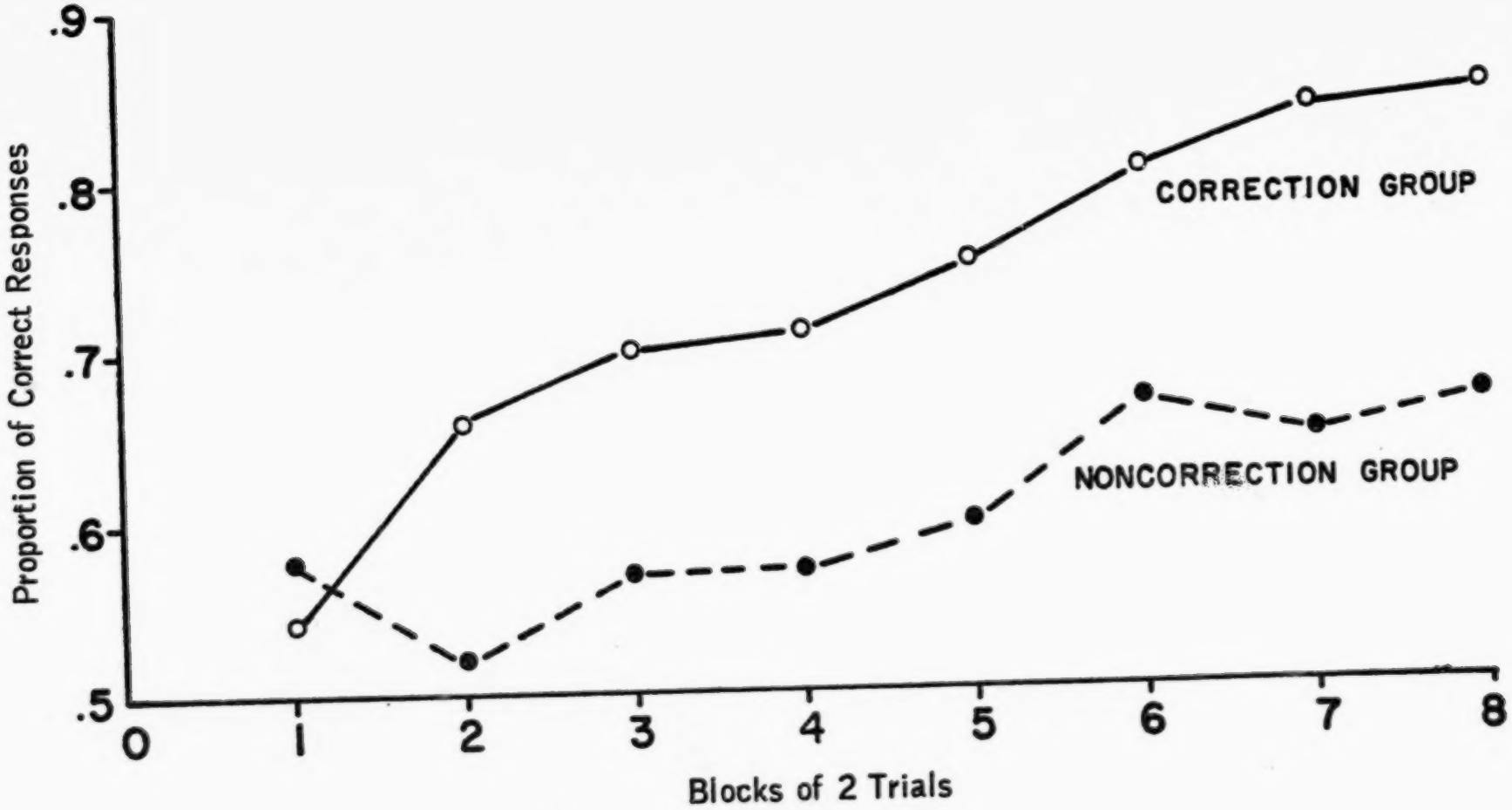


Fig. 1. Proportion of correct responses over all trials (Binary number experiment).

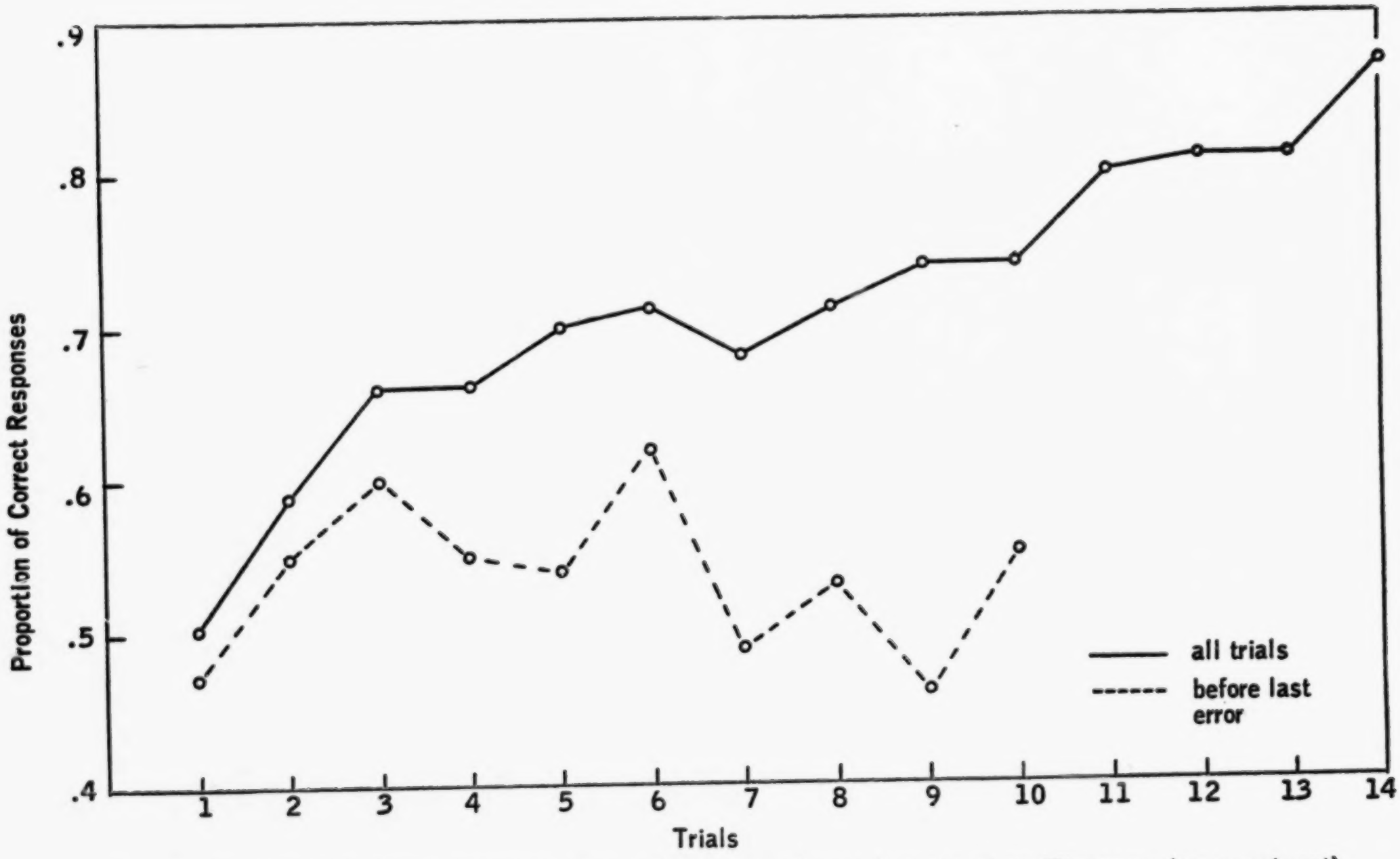


Fig. 2. Proportion of correct responses prior to last error and mean learning curve (Binary number experiment).

Using as the criterion that the concept had been learned if the responses to the last three presentations of each stimuli represented were correct, we divided the data into two parts. The data from the group meeting the criterion were arranged for concept learning analysis, in this case a two-item learning situation. The remaining data were assumed to represent paired-associate learning involving six independent stimulus items. For the paired-associate group over the first ten trials we had 81 cases. For the concept-formation group we had 21 cases with 48 trials in each. The χ^2 test of stationarity was not significant for either group (for the concept sub-group $\chi^2 = 8.36$, $df = 9$, $P > .30$, $N = 357$; for the paired-associate sub-group $\chi^2 = 11.26$, $df = 8$, $P > .10$, $N = 570$).

To provide a more delicate analysis of this important question of stationarity we can construct Vincent curves in the following manner (cf. Suppes and Ginsberg [1963]). The proportion of correct responses prior to the last error may be tabulated for percentiles of trials instead of in terms of the usual blocks of trials. In Fig. 3 the mean Vincent curve for the subjects in the binary number experiment who met the concept criterion is shown. This Vincent curve is plotted in terms of quartiles. As the mean percentile of each of the four quartiles is 12.5%, 37.5%, 62.5% and 87.5% respectively and C represents the 100% point, the distance between 4 and C on the abscissa is one-half of that between the quartiles themselves. The evidence for non-stationarity in the final quartile will be discussed subsequently along with some of the other Vincent curves shown in this figure.

Insert Figure 3 about here

It should be noted, of course, that the subjects who take longer to reach criterion are weighted more heavily in the Vincent curves. For example, suppose one subject has sixteen responses prior to his last error whereas another subject has four responses prior to his last error. The first subject contributes four responses to each quartile whereas the second subject contributes only one. This point will be discussed in more detail below. I turn now to the second experiment.

Experiment II. Equipollence and Identity of Sets.

This experiment was performed with Rose Ginsberg, and we have published some of the results in Suppes and Ginsberg [1963] but the detailed report of the experiment has not yet appeared. The learning tasks involved in this experiment were equipollence of sets and the two related concepts of identity of sets and identity of ordered sets.

Ninety-six first grade subjects were run, in four groups of twenty-four each. In Group 1 the subjects were required to learn identity of sets for fifty-six trials and then equipollence for a further fifty-six trials. In Group 2 this order of presentation was reversed. In Group 3 the subjects learned first identity of ordered sets and then identity of sets. In Group 4 identity of sets preceded identity of ordered sets. Following our findings in Experiment I, that is, that learning was more rapid when the child was required to make an overt correction response after an error, we included this requirement in Experiment II and most of the subsequent experiments reported below. Also in this experiment and those reported below as well, no stimulus display on any trial was repeated for an individual subject. This was done in order to guarantee that the learning of the

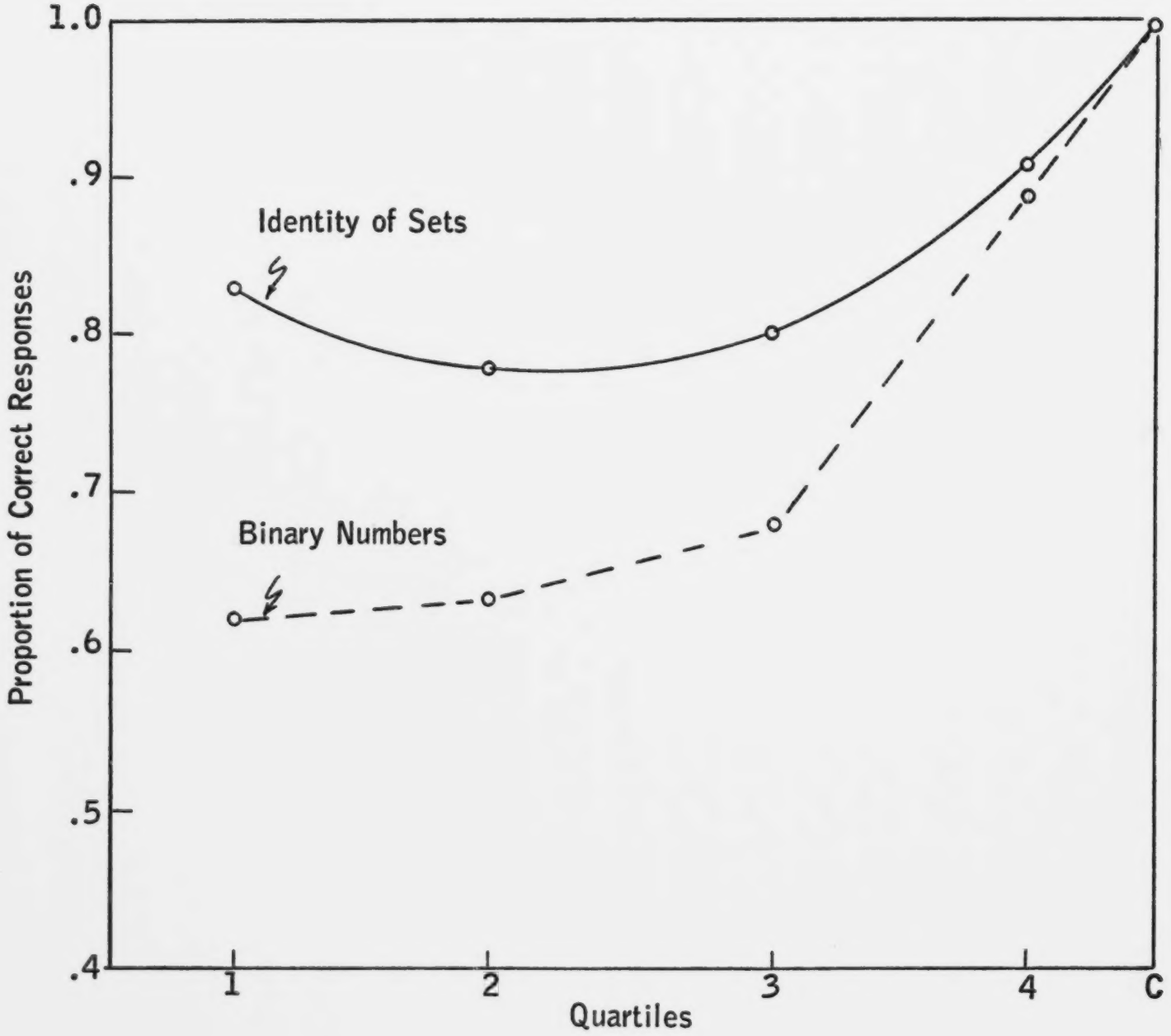


Fig. 3. Vincent learning curves in quartiles for proportion of correct responses prior to last error, Binary Numbers and Identity of Sets (Experiments I and II).

BLANK PAGE

concept could not be explained by any simple principles of stimulus response association, as was the case for Experiment I. For convenience of reference we have termed concept experiments in which no stimulus display is repeated pure property or pure concept experiments.

The sets depicted by the stimulus displays consisted of one, two or three elements. On each trial two of these sets were diplayed. Minimal instructions were given the subjects to press one of two buttons when the stimulus pairs presented were "the same" and the alternative button when they were "not the same".

Our empirical aims in this experiment were several. First, we wanted to examine in detail if the learning of simple set concepts by children of this age took place on an all-or-none conditioning basis. Secondly, as the two sequences of learning trials on two different concepts for each group would indicate we were interested in questions of transfer. Would the learning of one kind of concept facilitate the learning of another, and were there significant differences in the degree of this facilitation? Thirdly, we were concerned to find the behavioral level at which the concepts could be most adequately defined. For example, in learning the identity of sets could the learning trials be satisfactorily analyzed from the standpoint of all trials falling under a single concept? Would it be better to separate the trials on which identical sets were presented from those on which non-identical sets were presented in order to analyze the data in terms of two concepts? Or would a still finer division of concepts be called for in terms of sets identical in terms of order, sets identical as non-ordered sets, equipollent sets and non-equipollent sets?

In somewhat summary fashion the experimental results were as follows. First, the mean learning curves over all trials for all four groups are shown in Figs. 4-7. As is evident from these curves the

Insert Figures 4-7 about here

number of errors on the concept of identity of ordered sets was extremely small. From the high proportion of correct responses even in the first block of trials of this concept it is evident that this is a very natural and simple concept for children. Learning curves for trials before the last error are also shown in these figures. To identify the last error prior to conditioning, we adopted a criterion of 16 successive correct responses. For this reason these curves are only shown for the first 40 trials. The combined curve for Groups 1a and 4a is clearly stationary. This is also the case for 3a, 4b, 2b and 3b.[2/] The results of the x^2 test of stationarity for blocks of 4 trials are shown in Table 1 and confirm these graphic observations. Only the curve for 1b approaches significance. (No computation was made for 3a because of the small number of errors; the number of subjects in the final block of 4 trials is shown

Insert Table 1 about here

in the right-hand column of the table.)

I shall restrict myself to one Vincent curve for this experiment. The 48 subjects of Groups 1 and 4 began with the concept of identity of sets. Of the 48 subjects, 38 met the criterion of 16 successive correct

2/ "Group 1a" refers to the performance of Group 1 subjects on the first of their two tasks, 1b to performance on the second task, and similarly for 2a, 2b, 3a, 3b, 4a and 4b.

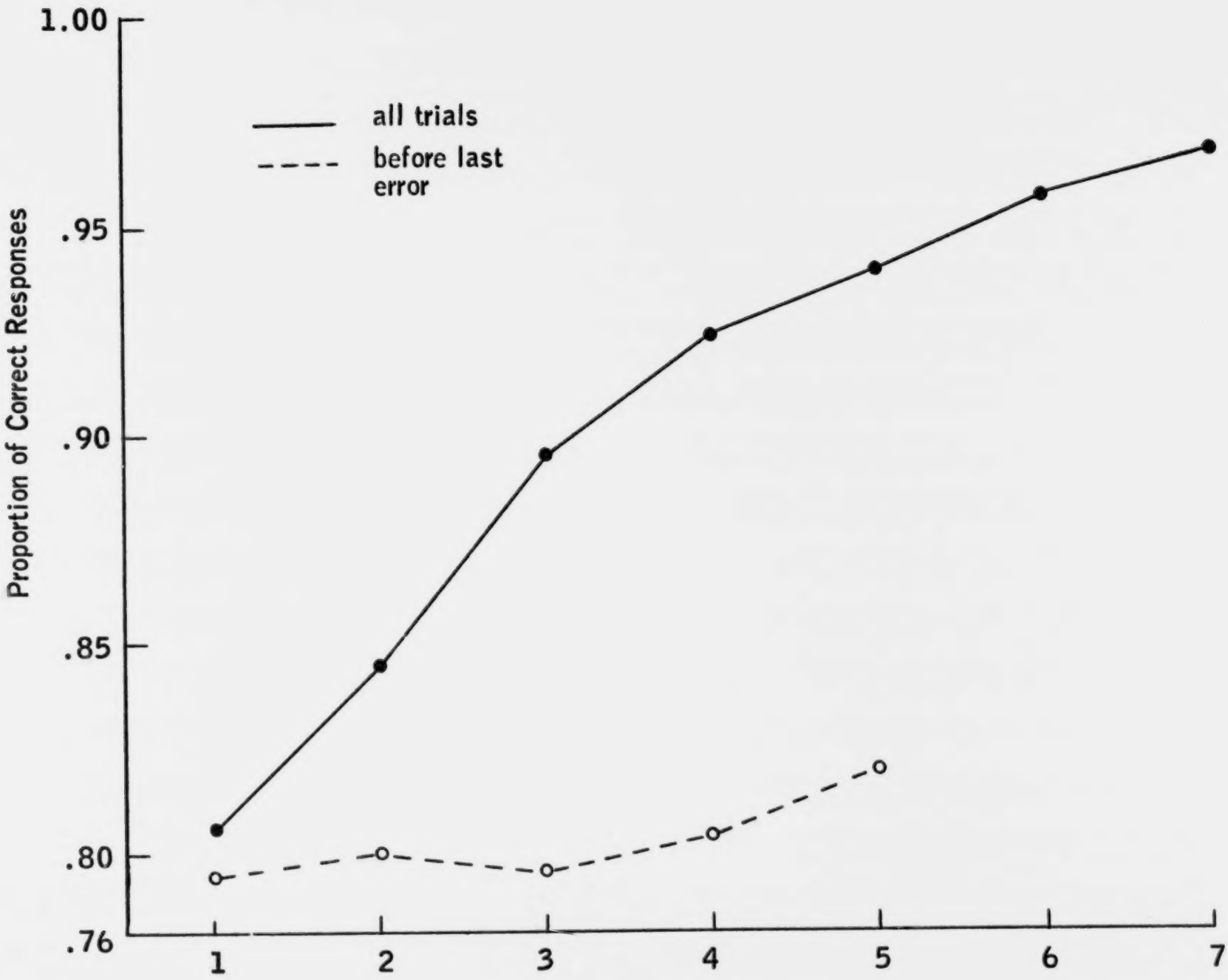


Fig. 4. Proportion of correct responses over all trials and before last error in blocks of 8 trials, identity of sets, N = 48, Groups 1a and 4a (Experiment II).

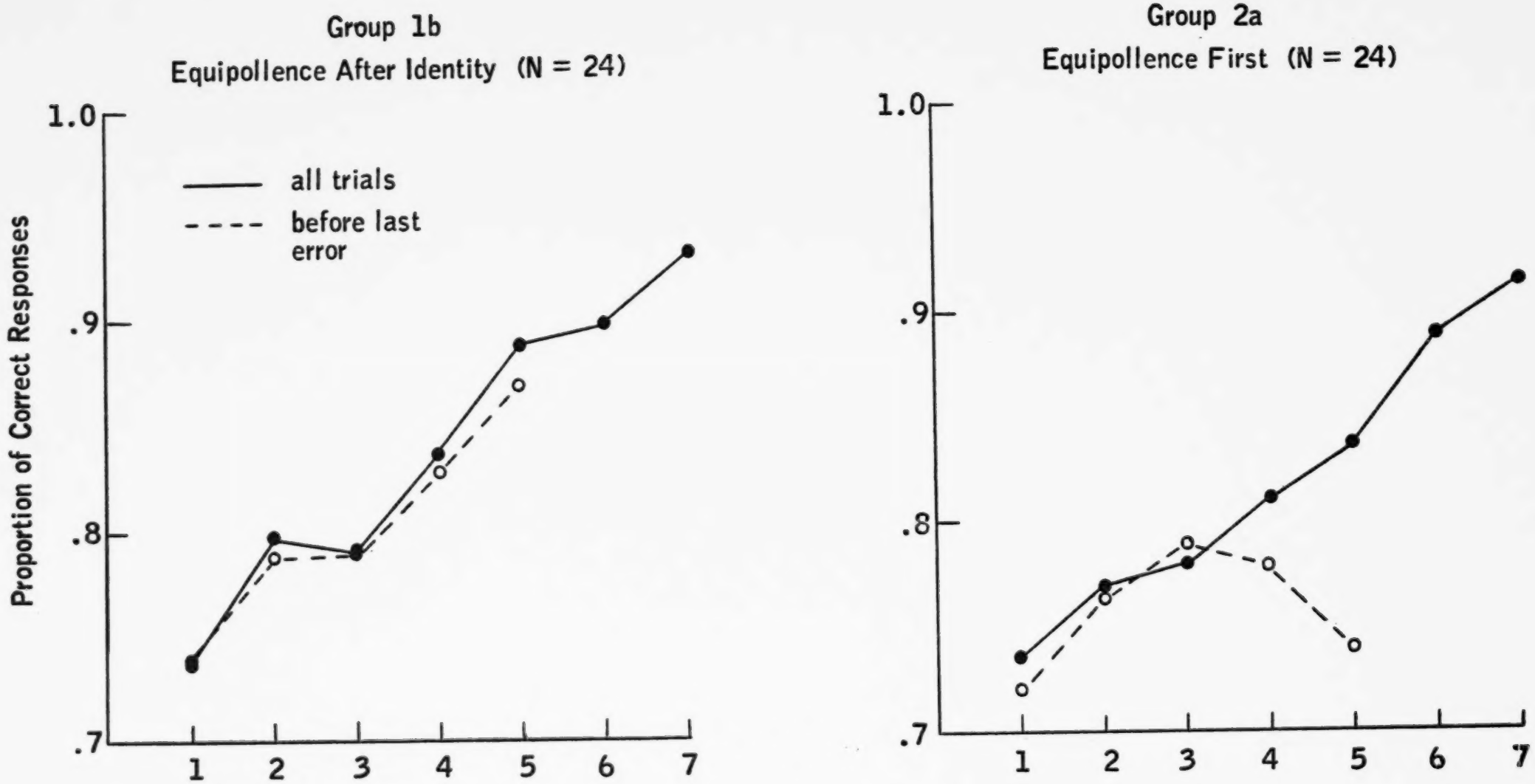


Fig. 5. Proportion of correct responses over all trials and before last error in blocks of 8 trials, equipollence of sets, Groups 1b and 2a (Experiment II).

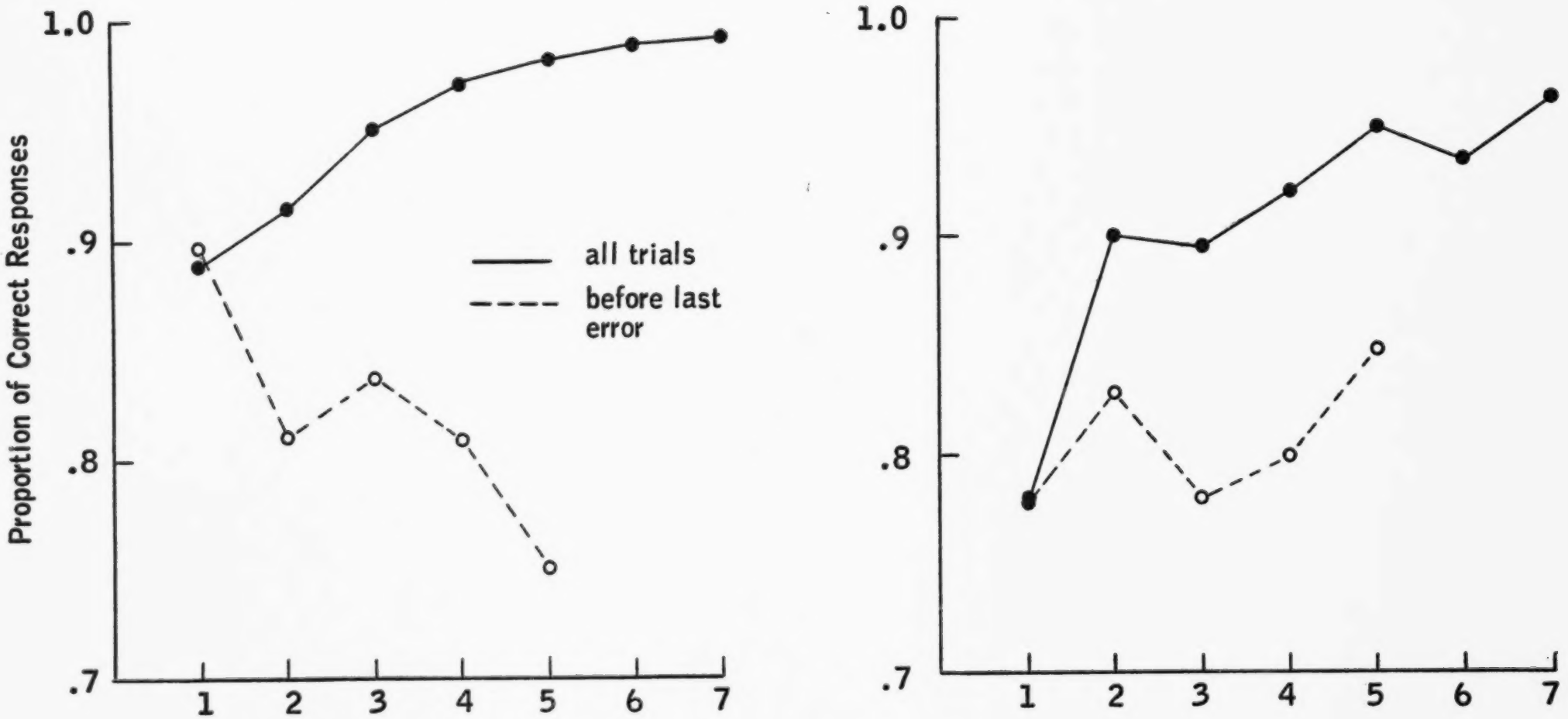


Fig. 6. Proportion of correct responses over all trials and before last error in blocks of 8 trials, identity of ordered sets, Groups 3a and 4b (Experiment II).

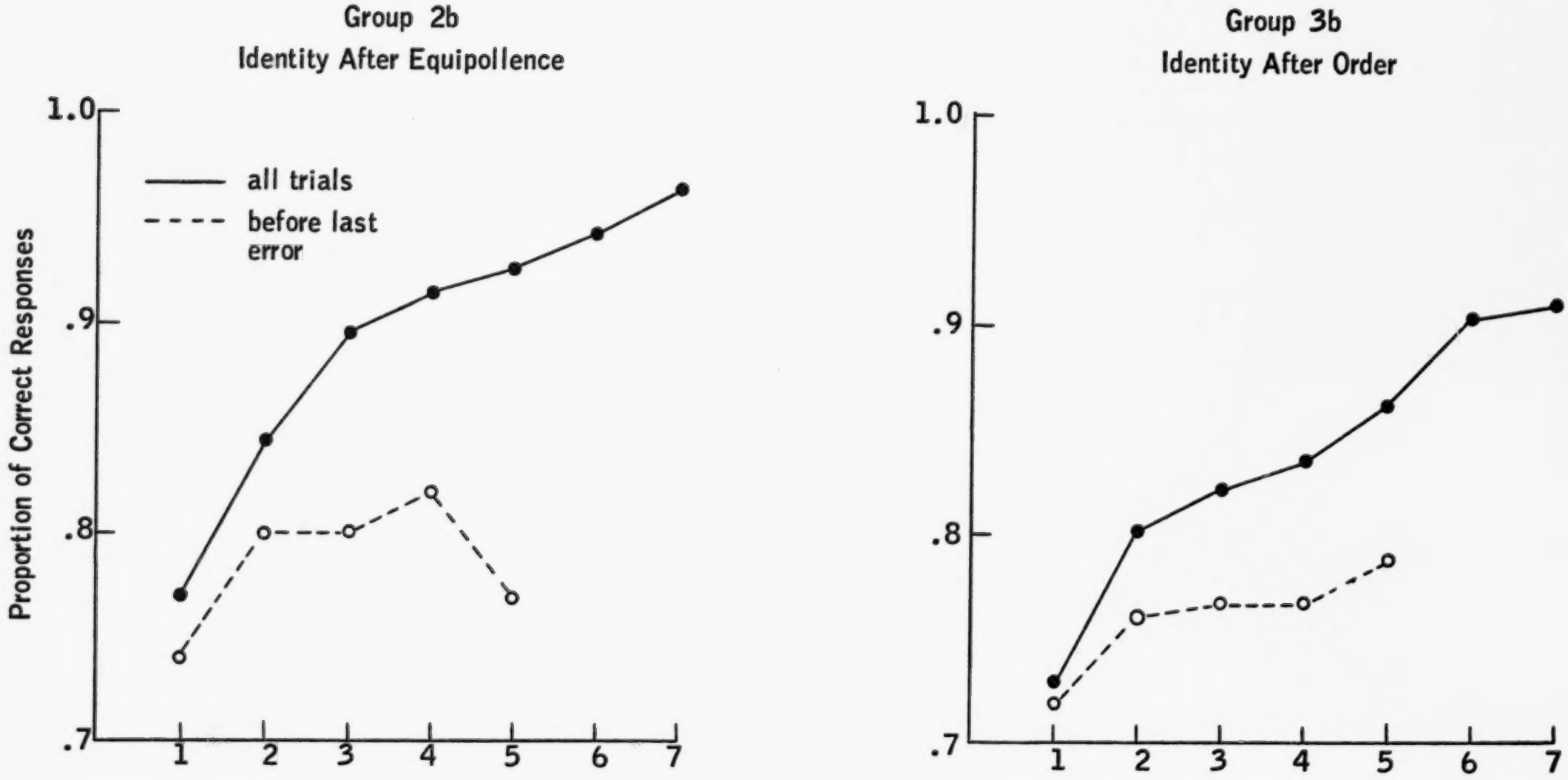


Fig. 7. Proportion of correct responses over all trials and before last error in blocks of 8 trials, identity of sets, Groups 2b and 3b (Experiment II).

Table 1

Stationarity Results for Equipollence and Identity of Sets Experiment (Exp. II)

Group	χ^2	df	P>	Ss in last block
1a & 4a	4.95	9	.80	9
1b	16.69	9	.05	12
2a	4.79	9	.80	11
3a	-- Too few errors --			1
4b	4.89	9	.80	5
2b	5.96	9	.70	5
3b	3.49	9	.90	10

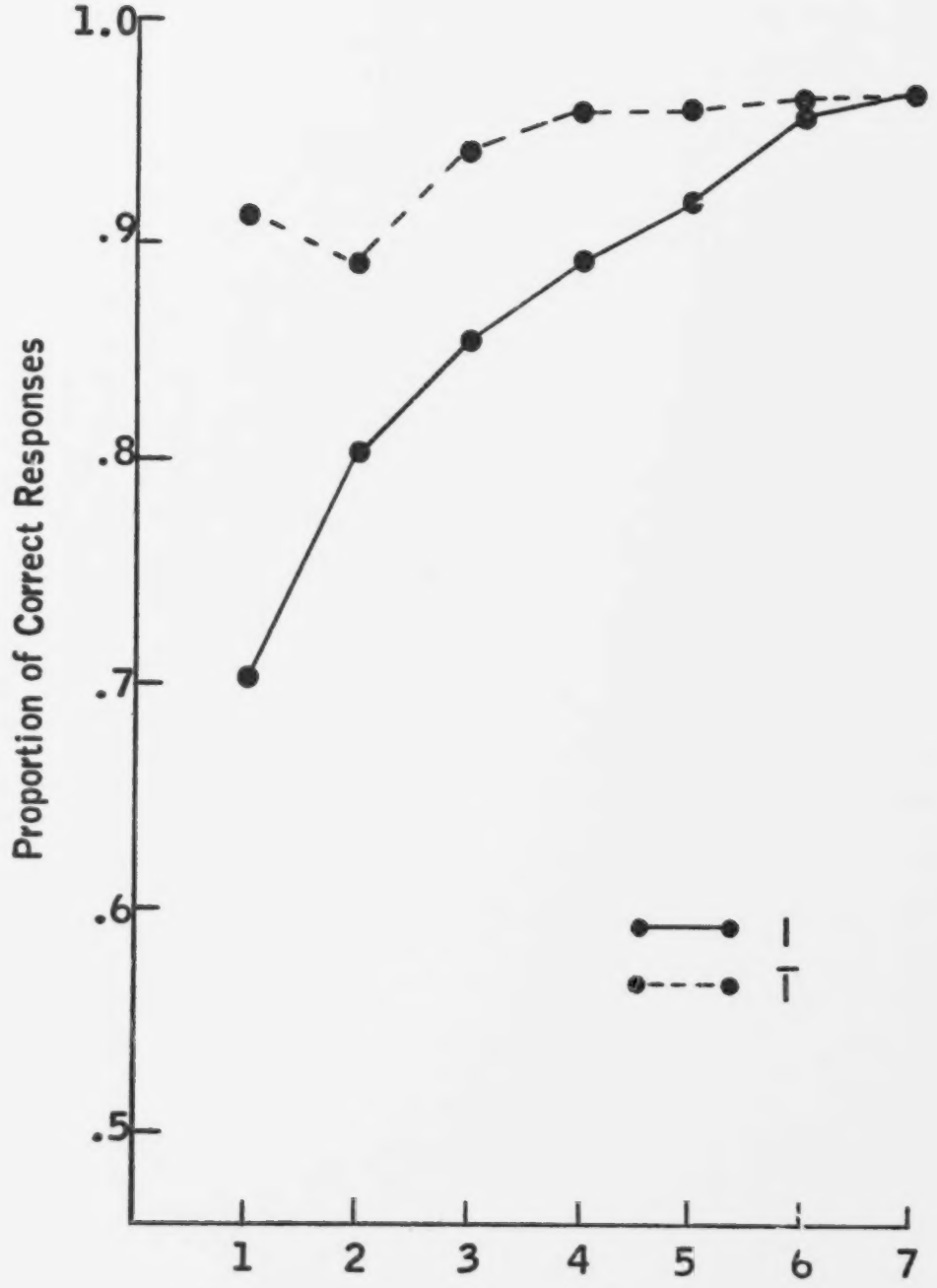
responses mentioned above. The Vincent curve for the criterion subjects is shown in Fig. 3. Evidence of non-stationarity in the fourth quartile is present as in the case of Experiment I.

From examination of the mean learning curves over all trials, there seems to be little evidence of transfer. And somewhat surprisingly, the only definite evidence confirms the existence of negative transfer. In particular, it seems clear from Fig. 6 that there is negative transfer in learning the concept of identity of ordered sets after the concept of identity of unordered sets. Also, from Figs. 4 and 7 it seems apparent that there is negative transfer in learning identity of sets after identity of ordered sets, but not after equipollence of sets.

The effects of transfer are actually more evident when we examine the data from the standpoint of 2 or 4 concepts. The mean learning curves over all 56 trials for the various concepts are shown in Figs. 8-14. The data points are for blocks of 8 trials. The abbreviations used in the legends are nearly self-explanatory. For the learning curves shown at the right of each figure, the O curve is for pairs of sets identical in the sense of ordered sets, the $I\bar{O}$ curve for pairs of sets identical only in the sense of unordered sets, the $E\bar{I}$ curve for pairs of equipollent but not identical sets, and the $\bar{E}$ curve for pairs of non-equipollent sets. These four curves thus represent all pairs of sets in four mutually exclusive and exhaustive classes. The legend is the same for all figures. On the other hand, the curves for the 2-concept analysis shown at the left of each figure differ in definition according to the problem being learned. In Fig. 8 the dichotomy is identical and non-identical sets (I and $\bar{I}$); in Fig. 9 it

is equipollent and non-equipollent sets (E and $\overline{E}$), and so forth for the other five figures.

Before considering questions of transfer, there are several remarks to be made about the individual figures. First, we observe that for each of the eight sub-groups (1a-4b) the learning curves for the 2-concepts and the 4-concepts are not homogeneous. A difference in difficulty at either level of analysis can be detected in all cases. Secondly, contrary to some experimental results in concept formation, the 2-concept curves at the left of each figure show that the absence of identity or equipollence is often easier than its presence. The situation seems to be this. The dichotomy of O vs. $\overline{O}$, i.e., identity or non-identity of ordered sets, is the natural one. When the "presence" of a concept disagrees with this natural dichotomy, as it does in the case of identity and equipollence of sets, it is more difficult than the absence of the concept. This conclusion is borne out by Figs. 8 and 10, for the groups beginning with identity and equipollence, respectively, as well as for Group 3b (Fig. 13), who were trained on ordered sets before identity of sets. This same conclusion even holds fairly well for the second sessions after training on some other concept (Figs. 9, 11 and 12). Fig. 14 which compares O and $\overline{O}$ after training on identity of sets indicates, I think, what tentative conclusion should be drawn. Whether the absence or presence of a concept is more difficult to learn depends much more on the previous training and experience of a subject than on the concept itself. When we compare Fig. 12 with Fig. 14 we see that even the difference between O and $\overline{O}$ in Fig. 12 is influenced by the prior training or identity, for the



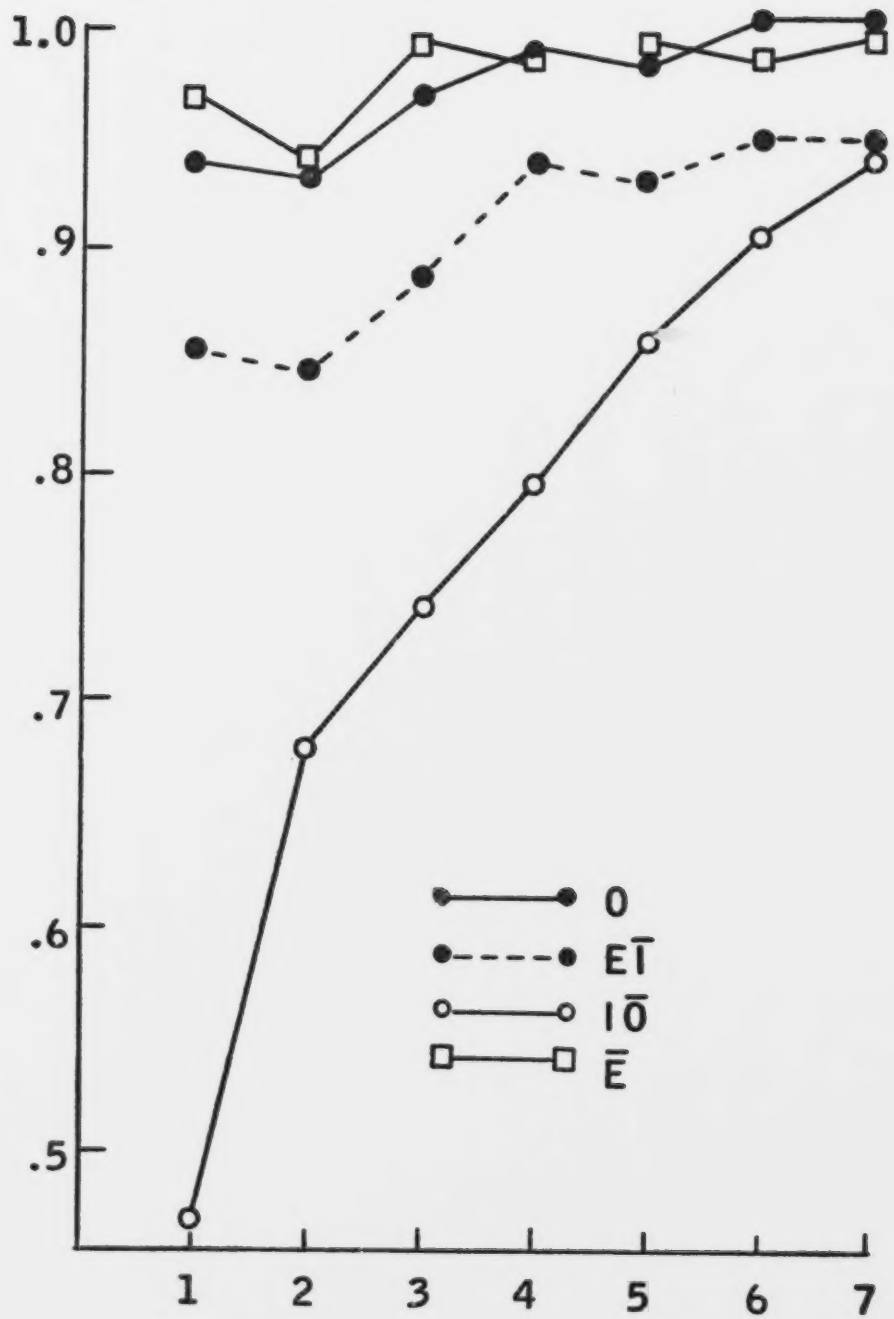


Fig. 8. Proportion of correct responses in blocks of 8 trials for 2 and 4 concepts, identity of sets (N = 48), Groups 1a and 4a (Experiment II).

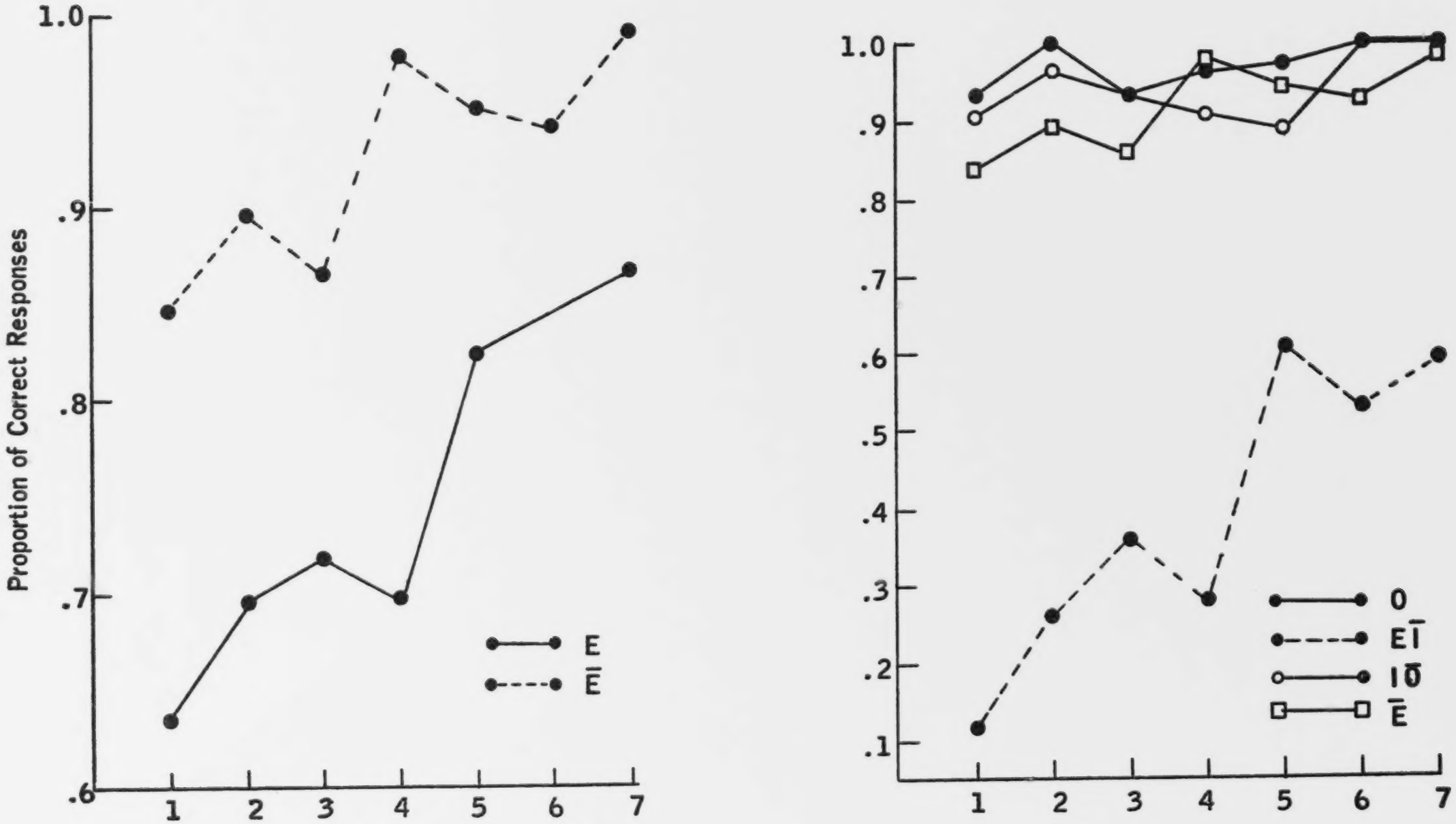
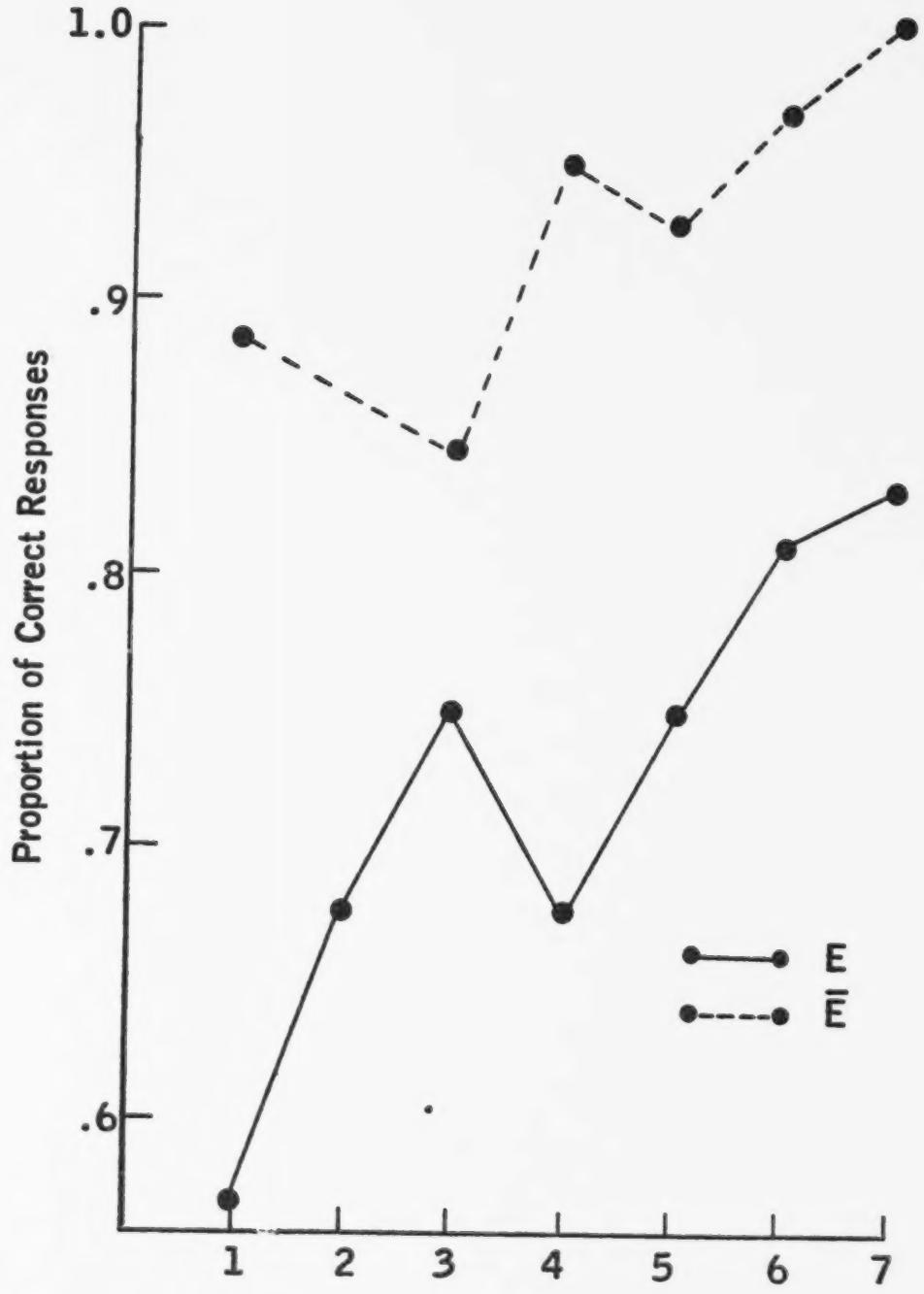


Fig. 9. Proportion of correct responses in blocks of 8 trials for 2 and 4 concepts, equipollence following identity of sets, Group 1b (Experiment II).



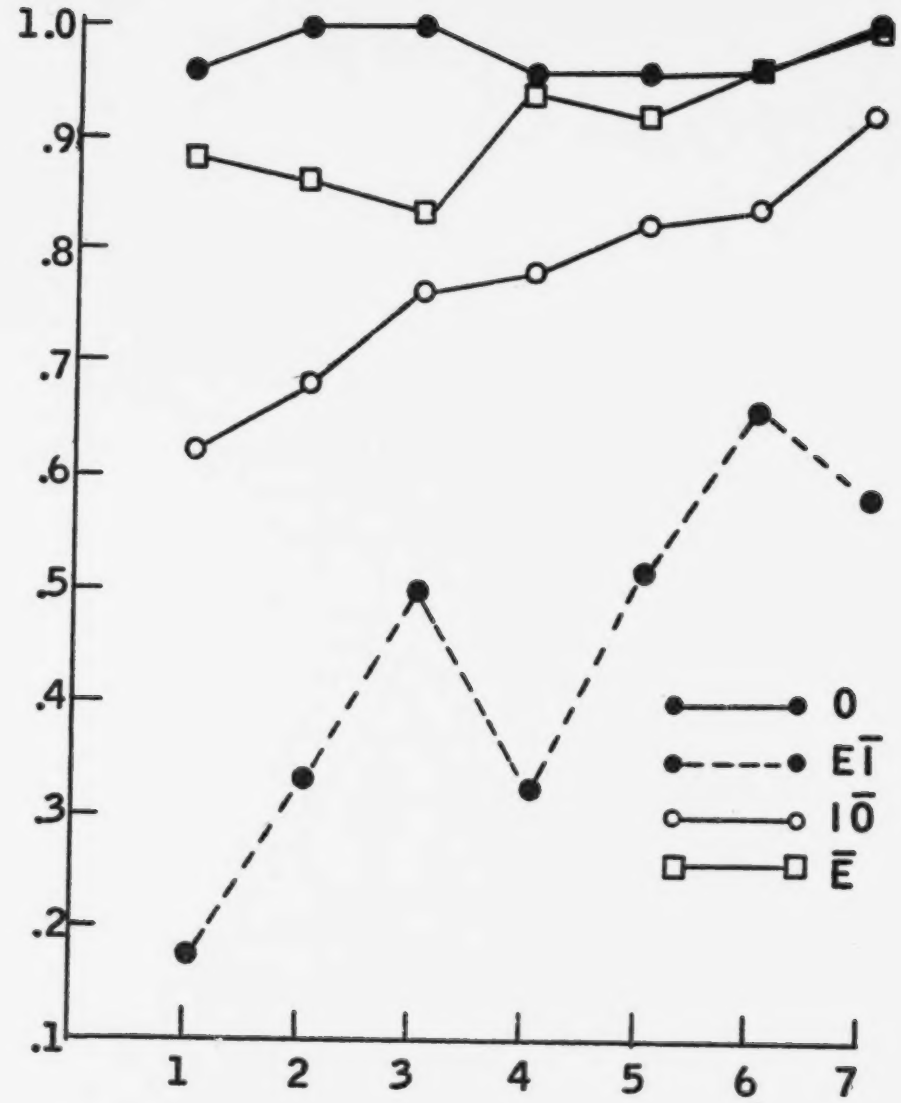
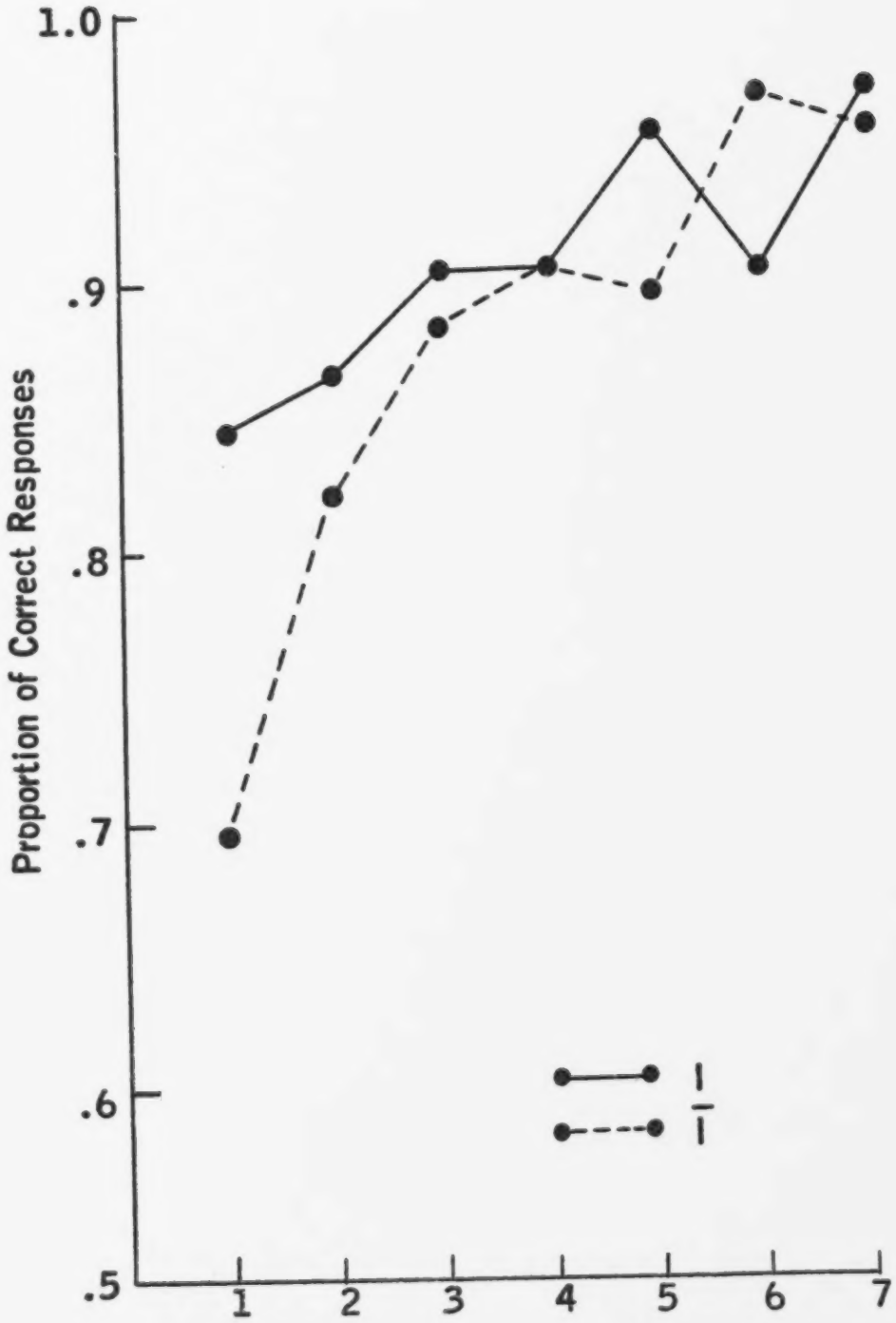


Fig. 10. Proportion of correct responses in blocks of 8 trials for 2 and 4 concepts, equipollence, Group 2a (Experiment II).


1.0
.9
.8
.7
.6
.5
Proportion of Correct Responses
1
2
3
4
5
6
7
I
Ī

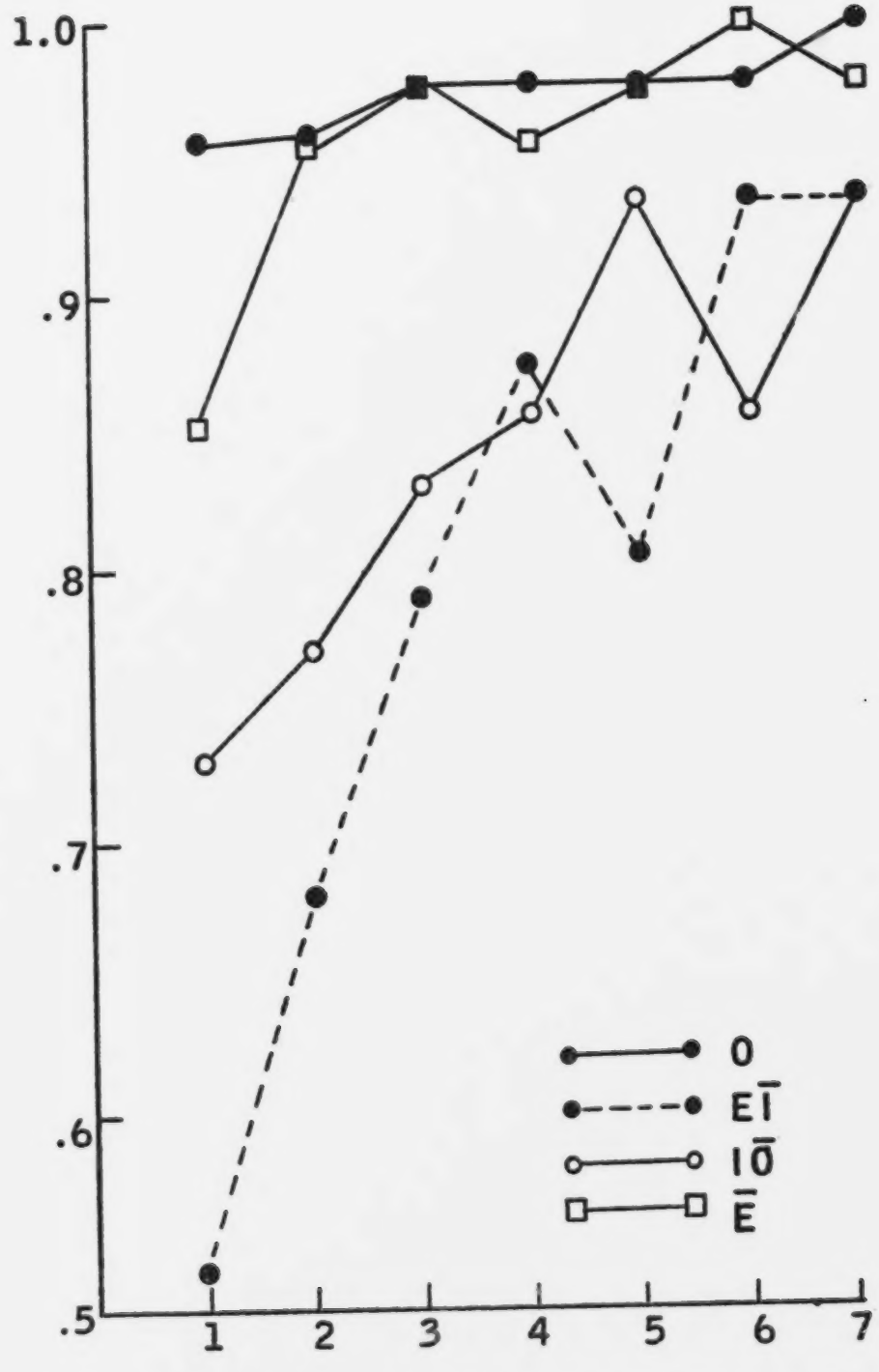

1.0
.9
.8
.7
.6
.5
1
2
3
4
5
6
7
O
EĪ
IŌ
Ē

Fig. 11. Proportion of correct responses in blocks of 8 trials for 2 and 4 concepts, identity of sets, Group 2b (Experiment II).

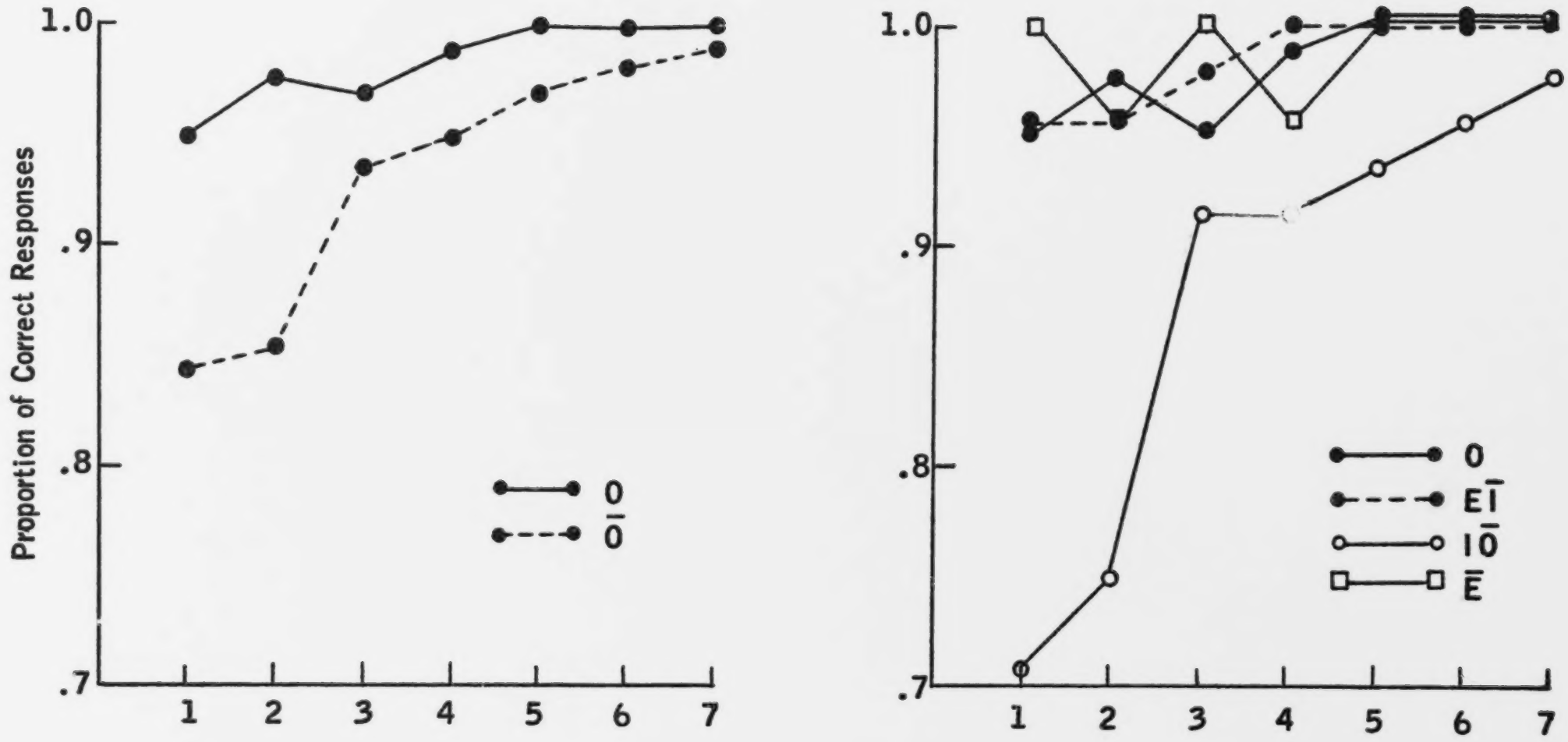


Fig. 12. Proportion of correct responses in blocks of 8 trials for 2 and 4 concepts, identity of ordered sets, Group 3a (Experiment II).

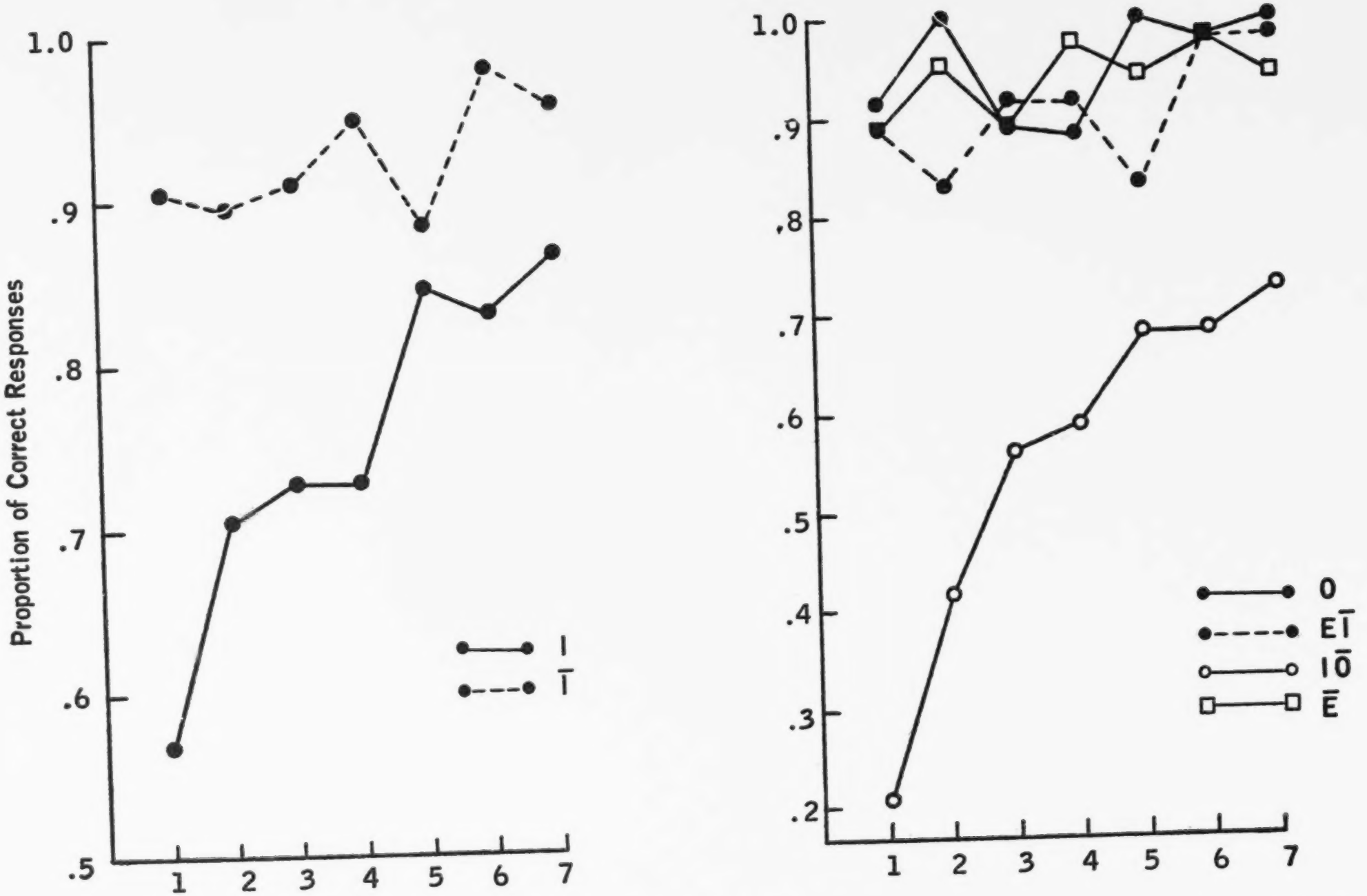


Fig. 13. Proportion of correct responses in blocks of 8 trials for 2 and 4 concepts, identity of sets following identity of ordered sets, Group 3b (Experiment II).

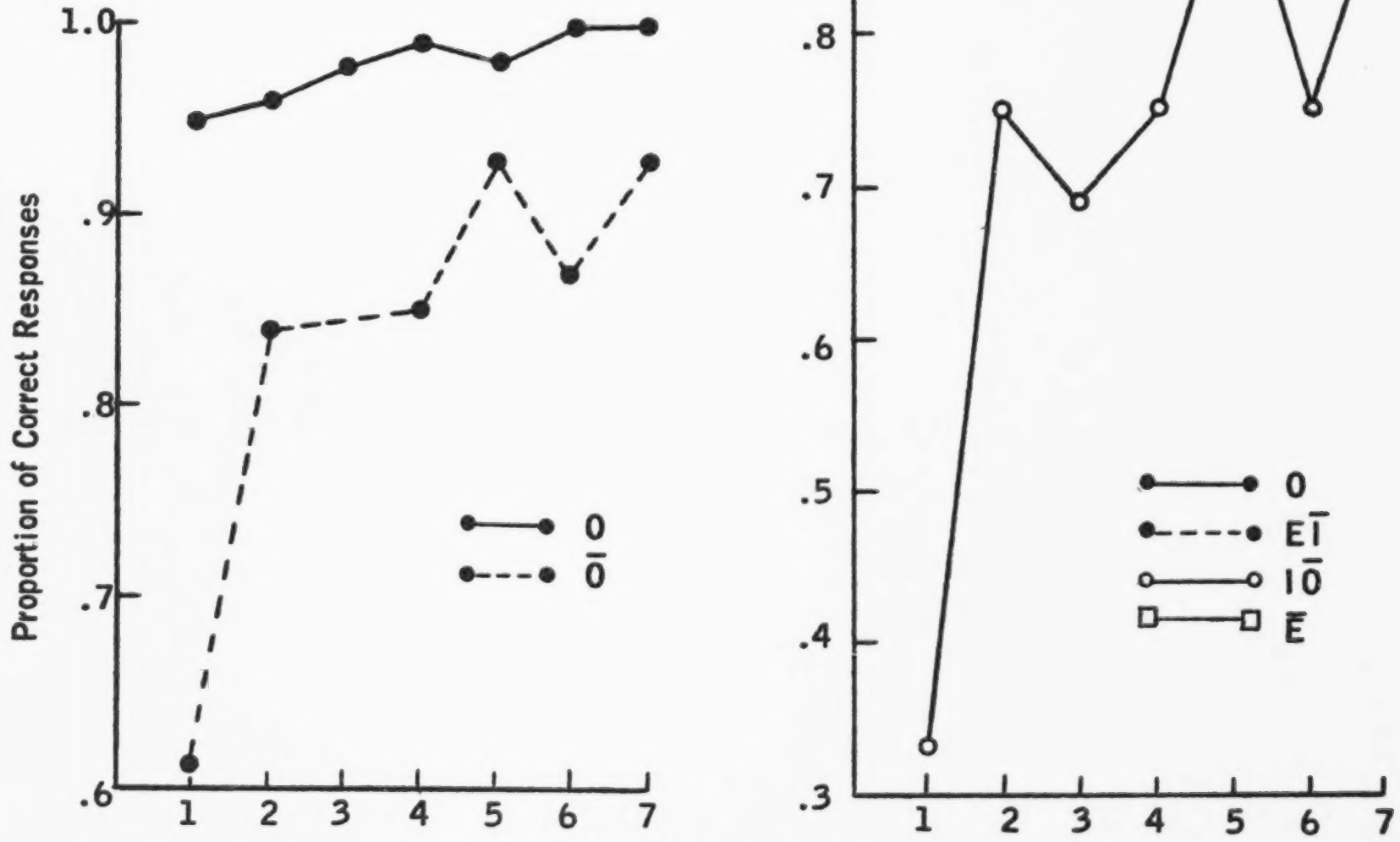


Fig. 14. Proportion of correct responses in blocks of 8 trials for 2 and 4 concepts, identity of ordered sets following identity of sets, Group 4b (Experiment II).

difference is greater in Fig. 14, and surely this is so because the $I\bar{O}$ cases have to be reversed in going from sets to ordered sets.

Thirdly, examination of the 4-concept curves reveals a natural gradient of difficulty. We may apply something rather like Coombs' [1950] unfolding technique to develop an ordinal generalization gradient. The natural or objective order of the classes of pairs of sets is O, $I\bar{O}$, $E\bar{I}$, $\bar{E}$. For any of the three concepts of sameness studied in the experiment, we may, without disturbing this objective ordering, characterize the classes exhibiting presence of the concept and those exhibiting its absence by cutting the ordering into two pieces. On a given side of the cut, as I shall call it, the nearer a class is to the cut the more difficult it is. Consider, to begin with, Fig. 8. The task is identity of sets, whence the cut is between $I\bar{O}$ and $E\bar{I}$, and we see that, on the one side $I\bar{O}$ is more difficult than O, and on the other side of the cut, $E\bar{I}$ is more difficult than $\bar{E}$. Turning to Fig. 9, the task is equipollence and thus the cut is between $E\bar{I}$ and $\bar{E}$. Of the three concepts on the $E\bar{I}$ side, $E\bar{I}$ is clearly the most difficult, and $I\bar{O}$ is slightly more difficult than O, sustaining the hypothesis of an ordinal gradient. In Fig. 10, the task is equipollence again, but in this case without prior training and the results are as they should be but more decisive than those shown in Fig. 9. Fig. 11, like Fig. 8, sustains the hypothesis when the task is identity of sets. In the case of Fig. 12 the task is identity of ordered sets, and thus $I\bar{O}$, $E\bar{I}$ and $\bar{E}$ occur on the same side of the cut. $I\bar{O}$ is clearly the most difficult, but it is not really possible clearly to distinguish $E\bar{I}$ and $\bar{E}$ in difficulty, for very few errors

are made in either class. In Fig. 13 the task is identity of sets again, but this time following identity of ordered sets. The proper order of difficulty is maintained but the distinction between $E\bar{I}$ and $\bar{E}$ is not as sharply defined as in Fig. 8 or Fig. 11. Finally in Fig. 14, the task is identity of ordered sets following identity of sets. The gradients are as predicted by the hypothesis and are better defined than in Fig. 12 -- no doubt because of the prior training on identity of sets. The existence and detailed nature of these natural gradients of difficulty within a concept task are subjects that seem to be worth considerable further investigation.

I turn now to evidence of transfer in the 4-concept analysis. From examination of the overall mean learning curves, which in the terminology of the present discussion are the 1-concept curves, we observed no positive transfer, but two cases of negative transfer. As might be expected, the 4-concept curves yield a richer body of results. Here I shall try to summarize only what appear to be the most important points. Comparing Fig. 8 and Fig. 11, we see that for learning of identity of sets, prior training on equipollence has positive transfer for class $I\bar{O}$ and negative transfer for $E\bar{I}$. The qualitative explanation seems obvious. The initial natural dichotomy seems to be O, $\bar{O}$, and for this dichotomy $I\bar{O}$ is a class of "different" pairs, but the task of equipollence reinforces the treatment of $I\bar{O}$ pairs as the "same". The situation is reversed for the class $E\bar{I}$, and thus the negative transfer, for under equipollence $E\bar{I}$ pairs are the "same", but under identity of sets they are "different".

Comparing now Fig. 8 and Fig. 13 where the task is again identity of sets but the prior training is on identity of ordered sets rather than equipollence, there is, as would be expected by the kind of argument just given, negative transfer for the class $I\bar{O}$. There is also some slight evidence of positive transfer for $E\bar{I}$.

Looking next at Figs. 9 and 10, we observe positive transfer for the class $I\bar{O}$ when the task is equipollence and the prior training is on identity of sets. What is surprising is the relatively slight amount of negative transfer for the class $E\bar{I}$.

Finally comparing Figs. 12 and 14 for which the task is identity of ordered sets, and in the latter case this task preceded by identity of sets, we observe negative transfer for the class $I\bar{O}$, as would be expected. The response curves for the other three classes are too close to probability one to make additional inferences, although there is a slight negative transfer for $E\bar{I}$ which cannot be explained on the principles stated above.

It seems apparent from these results that the analysis of transfer in the learning of mathematical concepts may often be facilitated if a fine-scale breakdown of the concepts in question into a number of subconcepts is possible. What is most needed is a quantitative theory to guide a more detailed analysis of the transfer phenomena.

Experiment III. Polygons and Angles.

This experiment is reported in detail in Stoll [1962], and I present some of the data here with her permission. The subjects were thirty-two kindergarten children who were divided into two equal groups. For both groups the experiment was a successive discrimination,

three-response situation, with one group discriminating between triangles, quadrilaterals and pentagons, and the other group discriminating between acute, right and obtuse angles. For all subjects a typical case of each form (i.e., one of the three types of polygons or three types of angles) was shown immediately above the appropriate response key. As in the case of Experiment II, no single stimulus display was repeated for any one subject. Stimulus displays representing each form were randomized over experimental trials in blocks of nine, with three of each type appearing in each block of nine. The subjects were run to a criterion of nine successively correct responses, but with not more than fifty-four trials in any one session.

For the quadrilaterals and pentagons the guessing probability prior to the last error was essentially the same, $\hat{p} = .609$ and $\hat{p} = .600$ respectively. Consequently the proportions of correct responses for the combined data are presented in blocks of six trials, together with the mean learning curve over all trials in Fig. 15. The corresponding data for the triangles are not presented because the initial proportion of correct responses was quite high and the subjects

Insert Figure 15 about here

learned to recognize triangles correctly very easily.

Fig. 16 presents the same curves for the combined data for the three types of angles although the guessing probability varied between the

Insert Figure 16 about here

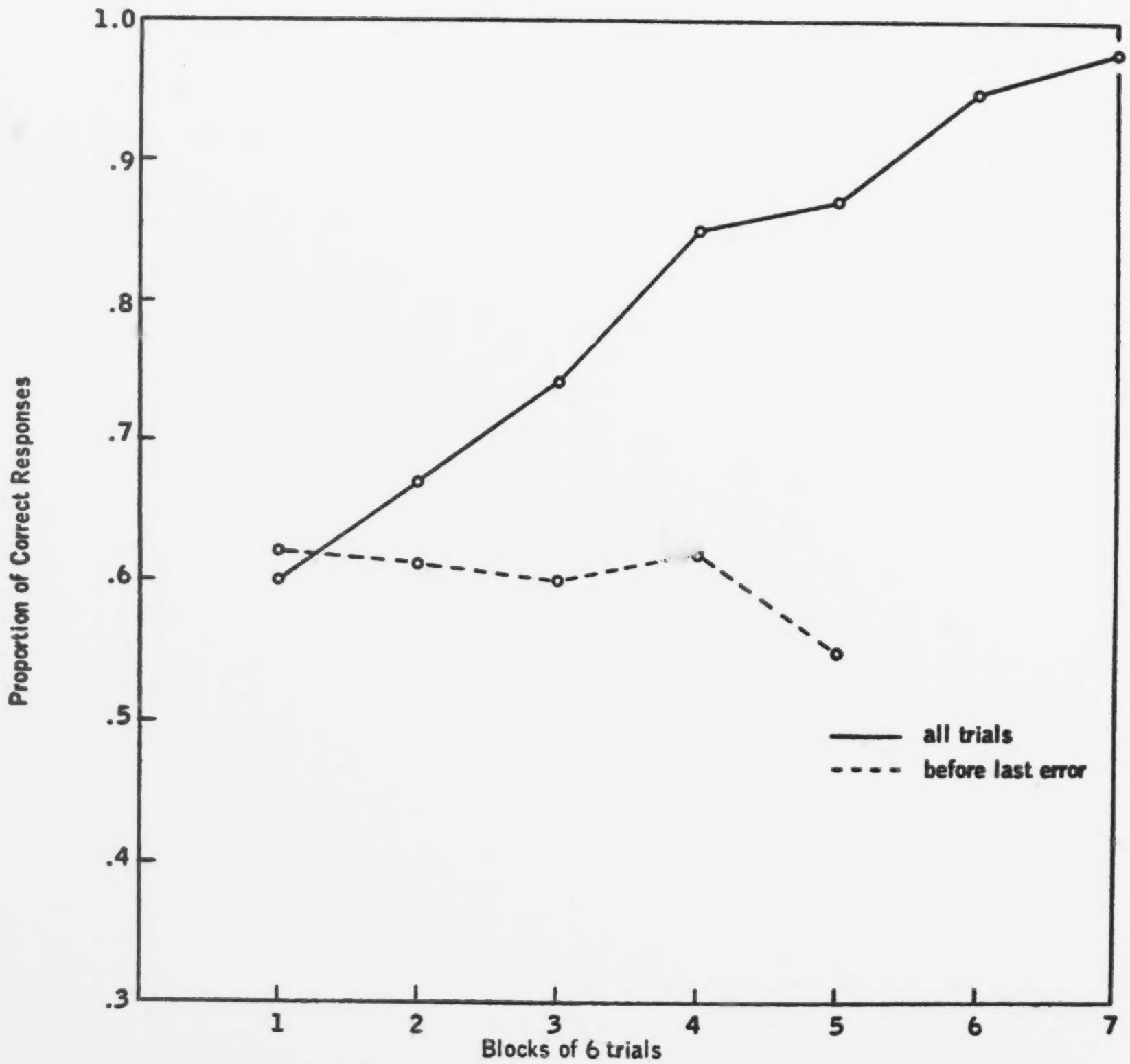


Fig. 15. Proportion of correct responses prior to last error and mean learning curve (Quadrilateral and pentagon concepts, Stoll experiment).

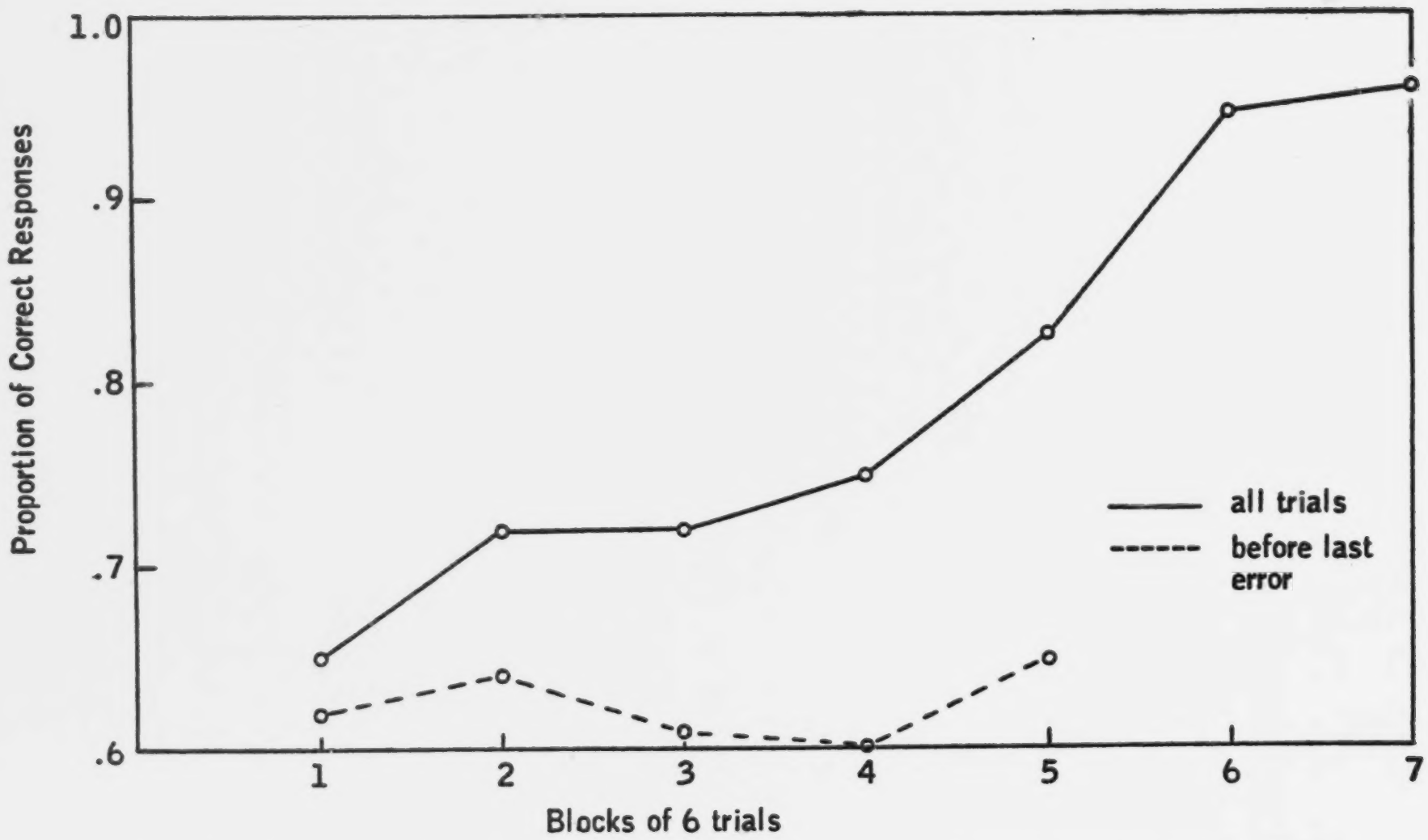


Fig. 16. Proportion of correct responses prior to last error and mean learning curve. (Acute, right and obtuse angle concepts, Stoll experiment).

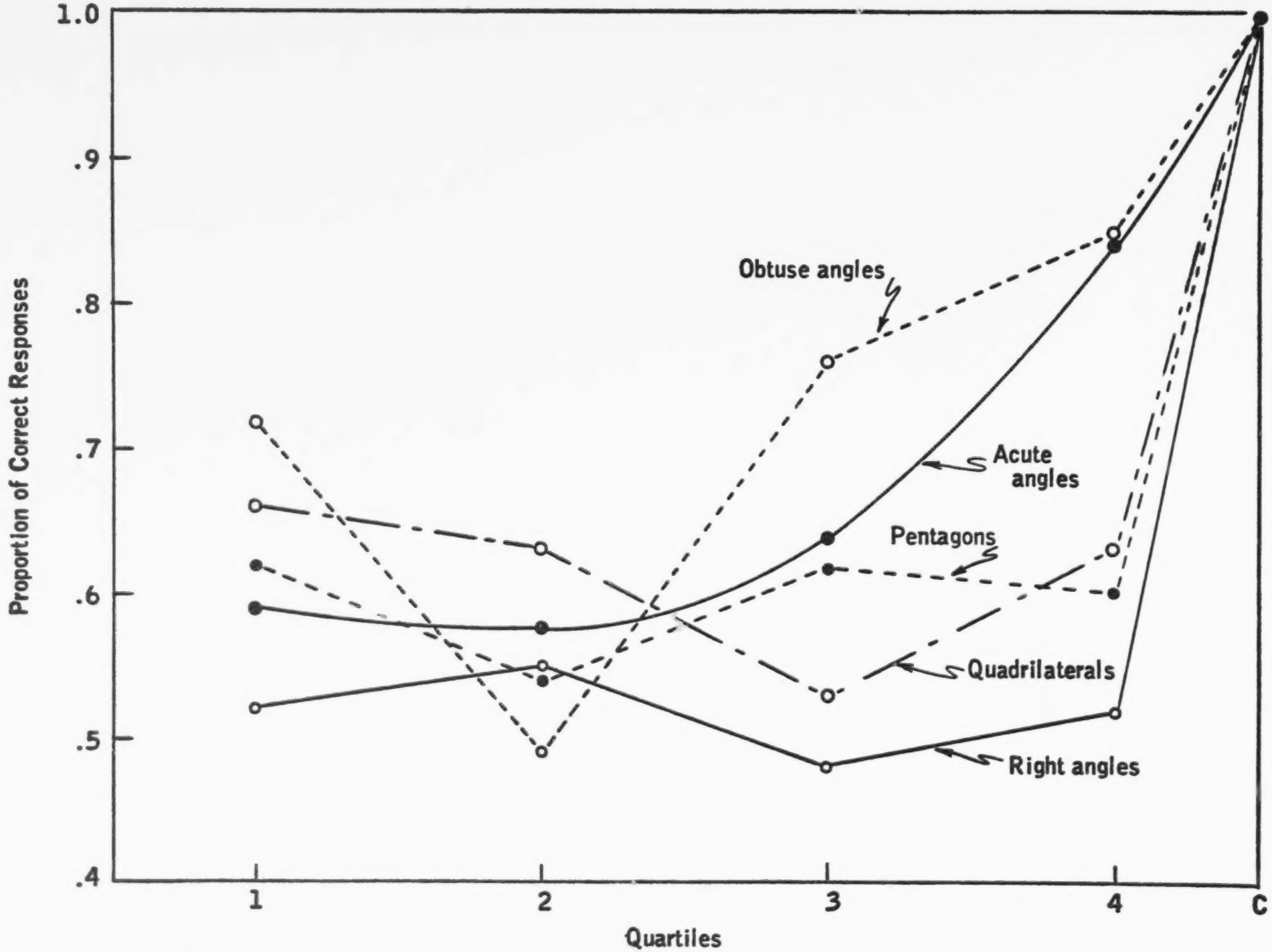


Fig. 17. Vincent learning curves in quartiles for proportion of correct responses prior to last error for Stoll experiment.

angles. Both figures strongly support the hypothesis of a constant guessing probability prior to conditioning. In the case of the quadrilaterals and pentagons, $\chi^2 = .71$, $df = 4$, $P > .90$, $N = 548$. In the case of the combined data for the angles, $\chi^2 = 0.97$, $df = 4$, $P > .90$, $N = 919$.

The Vincent curves for each of the concepts (except for the concept of the triangle) are shown in Fig. 17. The pentagons, quadrilaterals and right angles have quite stationary Vincent curves, whereas there is a definite increase in the fourth quartile of the Vincent curves for the acute and obtuse angles, and in fact in the case of the obtuse

Insert Figure 17 about here

angles there is a significant increase in the third quartile. Statistical tests of stationarity of these Vincent curves support these results of visual inspection. Each test has three degrees of freedom because the analysis is based on the data for the four quartiles. In the case of the quadrilaterals $\chi^2 = 1.75$, for the pentagons $\chi^2 = 1.33$, for the right angles $\chi^2 = .95$, for the obtuse angles $\chi^2 = 12.63$, and for the acute angles $\chi^2 = 16.43$. Only the last two values are significant.

Using responses before the last error, for all concepts except that of triangle, goodness-of-fit tests were performed for (i) stationarity in blocks of 6 trials, (ii) binomial distribution of responses as correct or incorrect in blocks of 4 trials, and (iii) independence of responses -- with the test being for zero-order vs. first-order

dependence. The results of these tests are presented in Table 2. The

Insert Table 2 about here

results shown strongly support the adequacy of the one-element model for this experiment.

Experiment IV. Variation in Method of Stimulus Display.

In this study conducted with Rose Ginsberg we compared the rate of learning in two experimental situations, one in which stimulus displays were presented individually in the usual way and the other in which the same stimulus displays were presented by means of colored slides to groups of four children. The concept to be learned was identity of sets, and in both situations the children were required to respond by pressing one of two buttons, depending upon whether the stimulus display on that trial was identical or non-identical. Of the sixty-four subjects thirty-two were from first grade and thirty-two from kindergarten classes. For the children receiving individual displays the experimental situation was essentially identical with that of Experiment II.

The mean learning curves for the two subgroups with random presentation are shown in Fig. 18. The results suggest that presentations by

Insert Figure 18 about here

Table 2

Stationarity, Order and Binomial Distribution Results
(Stoll Experiment on Geometric Forms).

	x^2	df	P >
Quadrilateral, p = .609			
Stationarity (N = 273)	1.68	4	.70
Order (N = 262)	0.65	1	.40
Binomial distribution (N = 65)	1.77	2	.40
Pentagon, p = .600			
Stationarity (N = 275)	2.40	4	.60
Order (N = 269)	1.76	1	.15
Binomial distribution (N = 65)	2.07	2	.35
Acute Angle, p = .674			
Stationarity (N = 338)	7.96	4	.05
Order (N = 348)	3.17	1	.05
Binomial distribution (N = 85)	2.66	2	.25
Right Angle, p = .506			
Stationarity (N = 313)	6.34	4	.10
Order (N = 326)	2.41	1	.10
Binomial distribution (N = 80)	10.52	2	.001*
Obtuse angle, p = .721			
Stationarity (N = 268)	1.10	4	.85
Order (N = 256)	7.32	1	.001*
Binomial distribution (N = 63)	2.90	2	.20
Quadrilateral and pentagon, p = ,604			
Stationarity (N = 548)	0.71	4	.90
Binomial distribution (N = 130)	1.77	2	.40
All angles, p = .624			
Stationarity (N = 919)	0.97	4	.90

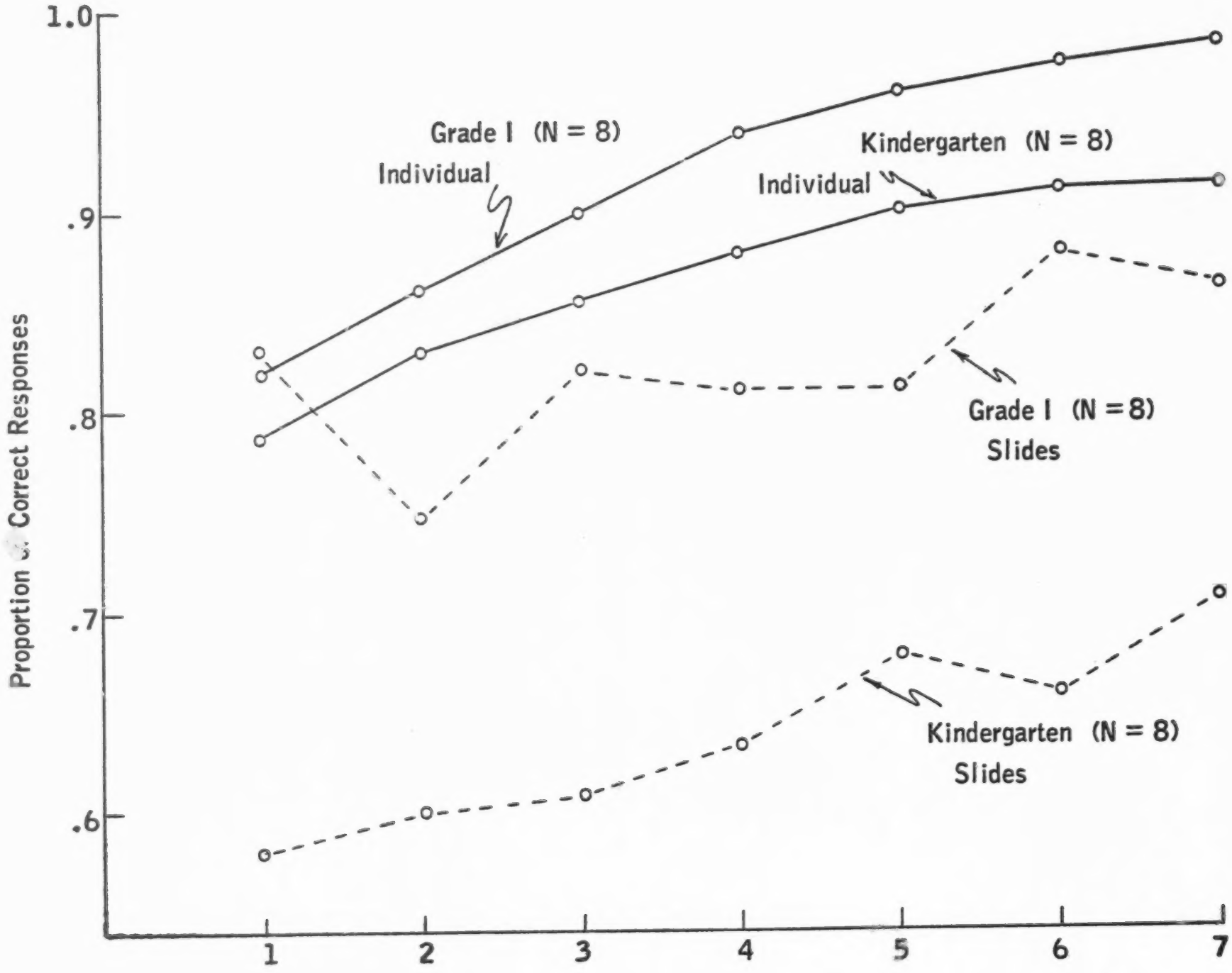


Fig. 18. Proportion of correct responses in blocks of 12 trials, subgroups with random presentation (Experiment IV).

slides is a less effective learning device for younger children, and the younger the child the more this finding seems to apply. At all levels of difficulty the kindergarten children learned more efficiently when the stimuli were presented to them in individual sessions. With one or two-element sets displayed Grade 1 subjects learned only slightly better in the individual session situation than in the slide situation, but when the task was more difficult (stimulus displays of three-element sets) the individual learning situation was clearly the most adequate. In interpreting these results it should be emphasized that the individual session was strictly experimental so that the amount of interaction between subject and experimenter was paralleled in both individual and slide situations.

Why these two experimental situations should produce different results in terms of efficiency of learning is not yet clear to us. One possibility is the following. It has been shown both with lower organisms (Murphy and Miller [1955]) and with young children (Murphy and Miller [1959]) that the ideal situation for learning is when the stimulus response and reinforcement are contiguous. In the individual sessions these requirements were met, for the response buttons were 1.5 in. below the stimulus displays and the reinforcement lights were 1.0 in. from the stimuli. On the other hand, in the slide presentations although the stimulus displays and reinforcements were immediately adjacent to each other, the response buttons were about 3 ft. from the screen on which the stimulus display was projected. Experimentally it has been shown (Murphy and Miller [1959]) that with children of this age group a separation of 6.0 in. is sufficient to interfere with efficient learning.

Experiment V. Incidental Learning.

This experiment represents a joint study with Rose Ginsberg. Thirty-six kindergarten children in three groups of twelve each were run for sixty trials a day on two successive days of individual experimental sessions during which they were required to learn equipollence of sets. On the first day the stimulus displays presented to the subjects on each trial differed in color between the three groups but were otherwise the same. In Group 1 all displays were in one color, black. In Group 2 equipollent sets were presented in red and non-equipollent sets in yellow. For the first twelve trials in Group 3 equipollent sets were red and non-equipollent sets in yellow, for the remaining forty-eight trials on that day the two colors were gradually fused until discrimination between them was not possible. On the second day all sets were presented to all three groups in one color, black.

As is apparent from the brief description of the experimental design, Group 1 simply had two days' practice under the same conditions with the concept of equipollence. In Group 2 the child did not actually need to learn the concept of equipollence but he could simply respond to the color difference on the first day, and it is well known that such a color discrimination for young children is a simple task. If the child in this group learned anything about equipollence of sets the first day, therefore, we may assume this to be a function of incidental learning. If incidental learning is effective, his performance on the second day when the color cue is dropped should at least be better than the performance of children in Group 1 on the first day. In Group 3

where we give the child the discriminative cue of color difference in the first trial and then very slowly withdraw that cue, the child should continue to search the stimulus displays very closely for a color stimulus, and is thus obliged to pay very close attention to the stimuli.

The mean learning curves for the three groups are shown in Fig. 19.

Insert Figure 19 about here

Of the three groups only Group 2 approaches perfect learning on the first day. In this group, of course, only color discrimination was necessary. Both the other groups did not improve over the first sixty trials, although Group 3 showed some initial improvement over those trials where the color cues remained discriminable. On the second day Group 1 showed no improvement, and the learning curves for this group and for Group 2 are practically identical. For Group 3 on the other hand, the results were conspicuously better on the second day than for those of any other group. It is apparent from these curves that the task chosen was relatively difficult for the age of the children, because essentially no improvement was shown by Group 1 over the entire 120 trials. The conditions in Group 3 where the children were forced to pay very close attention to the stimuli do seem to have significantly enhanced the learning.

Experiment VI. Variation of Response Methods.

This study was made jointly with Rose Ginsberg, and its object was to study the behavioral effects of different methods of response. Specifically, three groups each composed of twenty kindergarten children

were taken individually through a sequence of 60 trials on each of two successive days for a total of 120 trials. The task for all three groups was equipollence of sets.

In Group 1 the child was presented with pictures of two sets of objects and indicated by pressing one of two buttons whether the sets "went together" or did not "go together" (were equipollent or non-equipollent).

In Group 2 the child was presented with one display set and two "answer" sets and was required to choose the answer which "went together" with the display set.

In Group 3 the child was presented with one display set and three "answer" sets and made his choice from the three possible answers.

This situation has fairly direct reference to teaching methodology in the sense that Group 2 and Group 3 represent multiple-choice possibilities. In Group 1 where the child is required to identify either the presence of the concept or its absence on each trial, the situation is comparable to one in which the child is to indicate whether an equation or statement is correct or incorrect.

On the first day each group of children learned the task described above. On the second day they were run on an alternative method. Specifically, Group 1 was run under Group 3 conditions and Groups 2 and 3 were run under Group 1 conditions.

The mean learning curves for all groups on both days are shown in Fig. 20. It will be noticed that in Group 2, where the subjects were

Insert Figure 20 about here

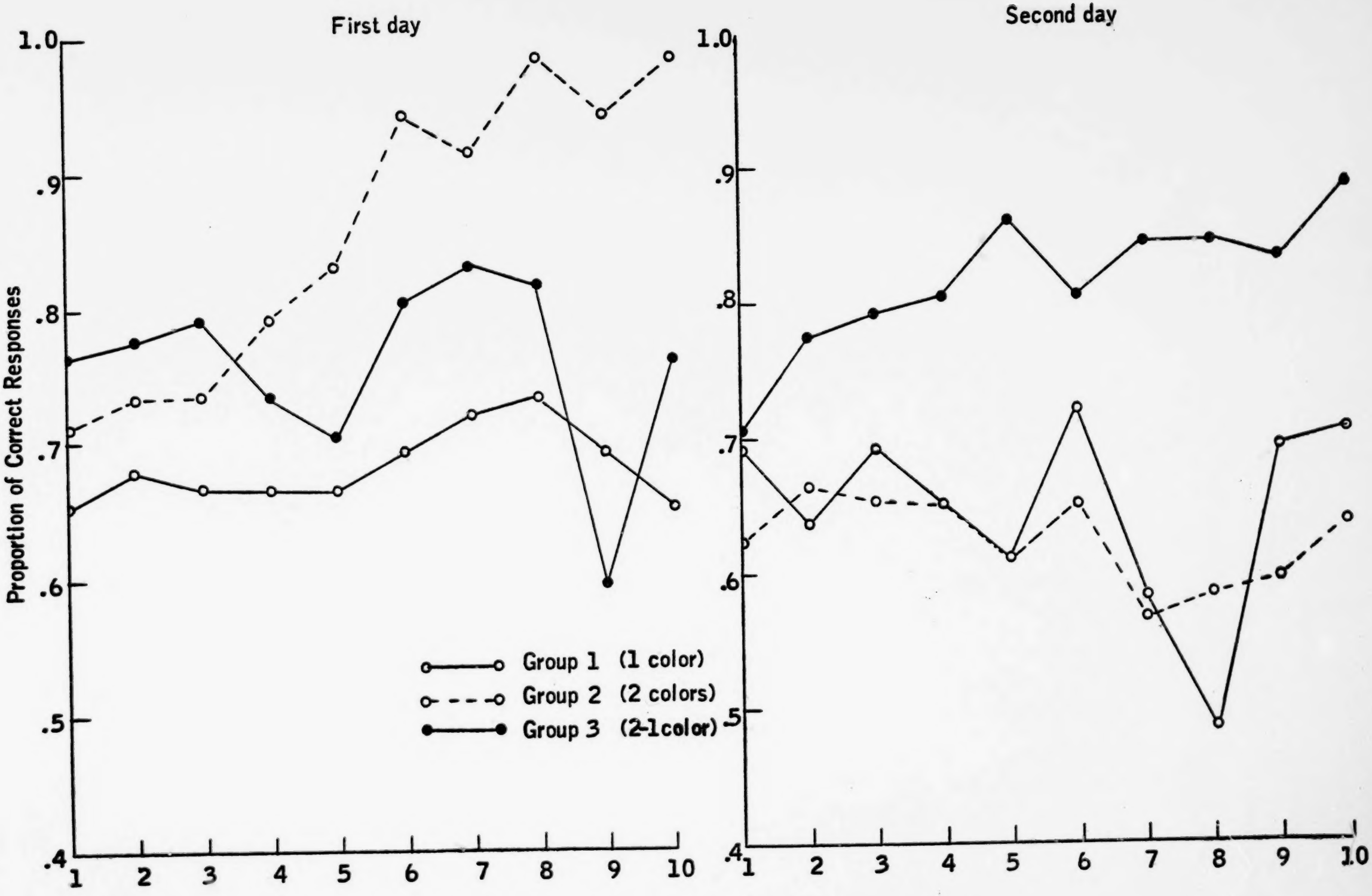


Fig. 19. Proportion of correct responses in blocks of 6 trials for both days (Experiment V).

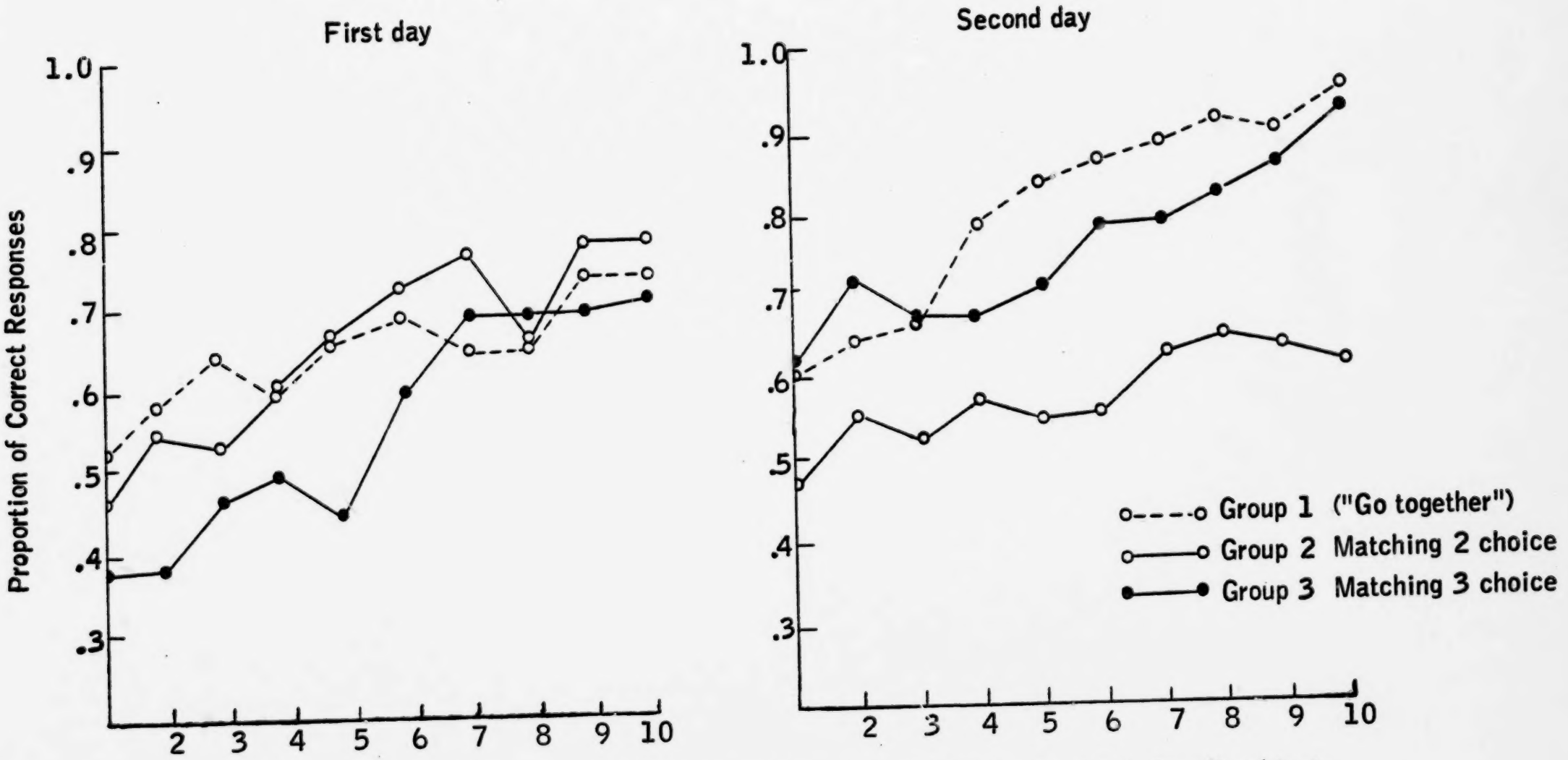


Fig. 20. Proportion of correct responses for two successive days in blocks of 6 trials for all subjects (Experiment VI).

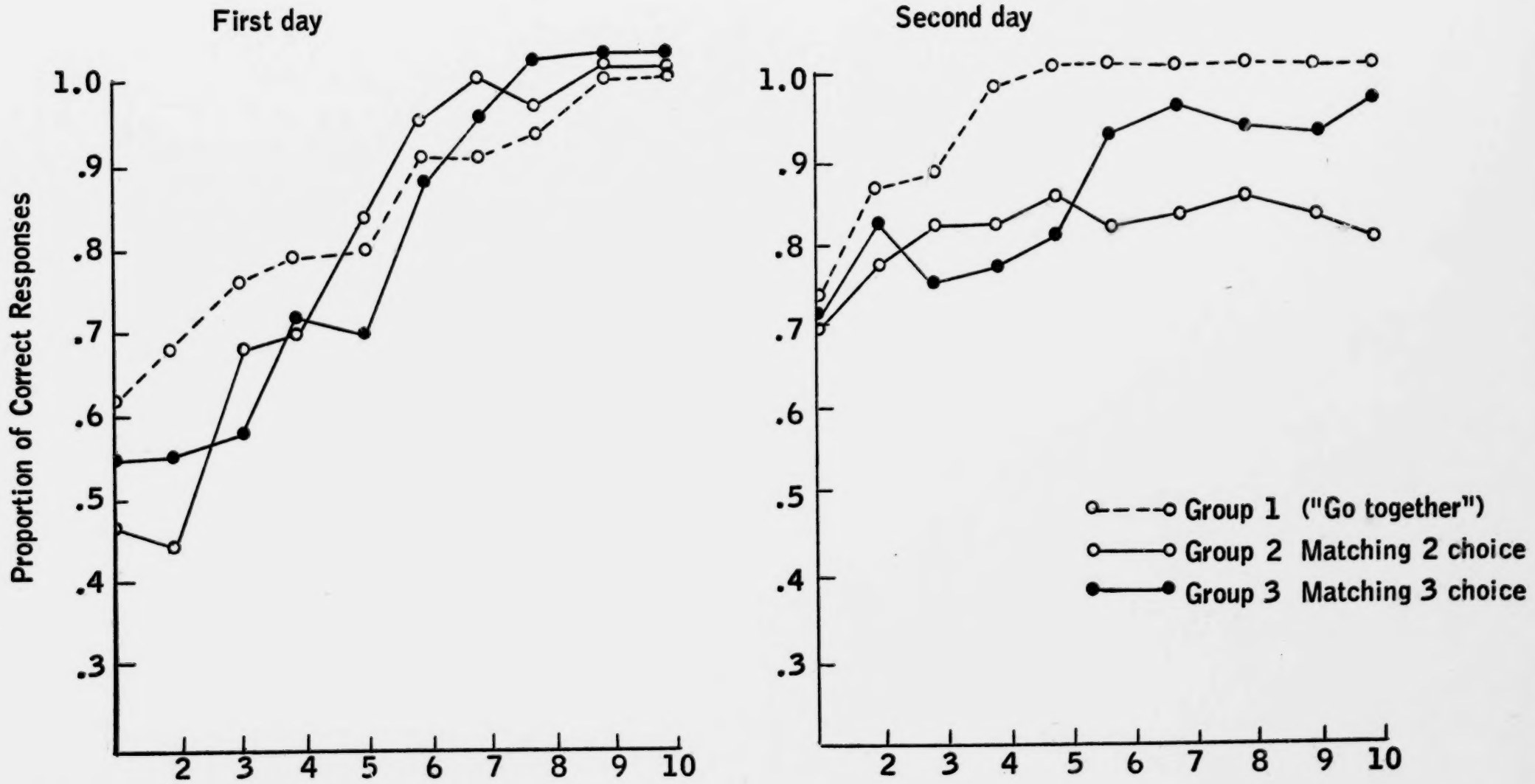


Fig. 21. Proportion of correct responses for two successive days in blocks of 6 trials for subjects achieving criterion of 12 successive correct responses (Experiment VI).

required to choose from one or two available responses, the subjects learned slightly more quickly and to a slightly better level of achievement on Day 1 than the other groups, but, on the second day, when the experimental conditions were shifted, Group 2 subjects did less well than the subjects in the other two groups. The clear superiority of Group 1 on the second day when they were transferred to Group 3 conditions indicates some positive transfer from learning to judge whether or not a concept is present to the multiple-choice situation, whereas the results for Groups 2 and 3 on the second day indicate some negative transfer from the multiple-response methods to the presence-or-absence method.

These results are further supported when we examine separately the data from subjects achieving a criterion of twelve successive correct responses on the first day. The more successful method was clearly that used in Group 1 as indicated from the curves in Fig. 21. The subjects in this group were conspicuously more successful than the

---

Insert Figure 21 about here

---

other groups on the second day, making, in fact, no errors on that day from Trial 30 to Trial 60. Group 3 achieved perfect scores on the second day only on the last six trials, and Group 2 never reached that level on the second day, although they, like the other criterion subjects, had achieved perfect learning on the first day.

It seems reasonable tentatively to conclude that the method used with Group 1, where subjects were required to recognize the presence or

absence of some property on each trial, is the more successful method in establishing understanding of a concept well enough to permit transfer to a different response method.

Support for the all-or-none model of conditioning is also to be found in this experiment. In Table 3, χ^2 goodness-of-fit tests of stationarity over trials before the final error for each group on each day are shown. The six values are all non-significant and thus support the basic assumption of the all-or-none models.

Insert Table 3 about here

Some Tentative Conclusions.

On the basis of the six experiments just discussed I would like to draw some tentative conclusions, some of which are important for pedagogical procedures (cf. Suppes and Ginsberg [1962b]). I want to emphasize, however, that I would not wish to claim that the evidence from these experiments is conclusive enough to establish any one of the six conclusions in any final way, but what I do hope is that the attempt to summarize some of the implications of these experiments will stimulate other research workers to investigate these and related propositions in more adequate detail.

1. Formation of simple mathematical concepts in young children is approximately an all-or-none process. Evidence indicates, however, that significant deviations from the all-or-none model are present (see the discussion of the two-element model below).

Table 3 Test for Stationarity over Trials before the Final Error (Experiment VI)

	Group 1		Group 2		Group 3	
	Day 1	Day 2	Day 1	Day 2	Day 1	Day 2
χ^2	4.97	2.41	10.76	4.255	16.07	2.87
df	8	1	9	8	9	7
P>	.70	.10	.20	.80	.05	.80

2. Learning is more efficient if the child who makes an error is required to make an overt correction response in the presence of the stimulus to be learned (Exp. I).
3. Incidental learning does not appear to be an effective method of acquisition for young children and in Exp. IV the group of children who responded to a color discrimination did not subsequently give any indication of having learned the underlying concepts.
4. Contiguity of response, stimulus,and reinforcement enhances learning (Exp. V).
5. In the learning of related mathematical concepts the amount of overall transfer from the learning of one concept to another is surprisingly small. However, considerable positive or negative transfer between specific sub-concepts is often present (Exp. II).
6. Transfer of a concept is more effective if the learning situation has required the subject to recognize the presence or absence of a concept in a number of stimulus displays than if learning has involved matching from a number of possible responses (Exp. VI).

Several of these conclusions are at variance with generally accepted results for adult learning behavior, for example, the efficacy of an immediate overt correction response (see Burke, Estes and Hellyer [1954] for negative results on this method in adults), the variation of response method, or the relative specificity of learning of concepts with relatively little transfer. What is much needed is a wider range of

systematic studies to isolate the factors of learning in young children that are particularly distinct from common features of adult learning behavior.

Two-Element Model.

In the first conclusion mentioned above we stated that the formation of concepts is approximately an all-or-none process in young children. On the other hand, the detailed analysis of responses prior to the last error indicates that in many cases there is an incremental effect appearing in the last quartile or even sometimes in the last two quartiles of the data. This matter is discussed in some detail in Suppes and Ginsberg [1963]). Here I would simply like to briefly mention what currently appears to be the best extension of the one-element model to account for these results.

The simplest alternative model is the linear incremental model with the single operator. The intuitive idea of this model is precisely the opposite of the all-or-none conditioning model. The supposition is that learning proceeds on an incremental basis. Let q_n be the probability of an error on trial n. Then the model is formulated by the following recursive equation:

$$q_{n+1} = (1-\theta)q_n ,$$

where $0 < \theta \leq 1$. It is simple to show but somewhat surprising that this purely incremental model has precisely the same mean learning curve as the all-or-none model if we set $c = \theta$. (To obtain this identity of the learning curves we must, of course, consider all responses and not simply responses prior to the last error.) The incremental model

does differ sharply from the all-or-none model in the kind of learning curve predicted for responses prior to the last error, as is evident from equation (1). Moreover, it may be shown that the concave upward Vincent curves obtained in several of the experiments discussed above (see Fig. 3 and Fig. 17) cannot be accounted for by the linear incremental models.

The second simple alternative, which will account for these concave upward Vincent curves, is a model which represents a kind of compromise between the all-or-none model and the incremental model. It results from a simple extension of the one-element model, namely, it is assumed that associated with each situation are two stimulus elements and, therefore, that the learning proceeds in two stages of all-or-none conditioning. Each of these two unobserved elements is conditioned on an all-or-none basis but the two parameters of conditioning, one for each element, may be adjusted to produce various incremental effects on the response probabilities. Let σ and τ be the two elements. The basic learning process may be represented by the following four-state Markov process where the four states (σ,τ), σ, τ, and 0 represent the possible states of conditioning of the two-stimulus elements.

	(σ,τ)	σ	τ	0
(σ,τ)	1	0	0	0
σ	$b'/2$	$1-b'/2$	0	0
τ	$b'/2$	0	$1-b'/2$	0
0	0	$a/2$	$a/2$	$1-a$

Because we do not attempt experimentally to identify the stimuli σ and τ, this Markov process may be collapsed into a three-state process, whose states are simply the number of stimuli conditioned to the correct response. In the matrix shown above a is the probability of conditioning at the first stage and b' is the probability of conditioning at the second stage. The division by 1/2 in the matrix simply represents the equal probability of sampling one of the two elements. If we consider only the number of stimuli, it is convenient to replace $\frac{b'}{2}$ by b and we obtain the transition matrix shown below:

	2	1	0
2	1	0	0
1.	b	1-b	0
0	0	a	1-a

To complete the description of the process we associate with the sampling of each element σ and τ a guessing probability g_σ and g_τ when the elements are still unconditioned. For the states 0 and 1 of the second matrix shown we then have the guessing probabilities g_0 and g_1 defined in the obvious manner in terms of the sampling probabilities:

$$g_0 = \frac{1}{2} g_\sigma + \frac{1}{2} g_\tau$$

$$g_1 = \frac{1}{4} g_\sigma + \frac{1}{4} g_\tau + \frac{1}{2} = \frac{1}{2} g_0 + \frac{1}{2} \quad .$$

The probabilities g_σ and g_τ are not observable but g_0 is and g_1 is a simple function of it. This means that we now have a process with three free parameters, the conditioning parameters a and b and the

guessing probability g_0. I shall not attempt to report here detailed application of this two-element model but we are now in the process of applying it to a number of different experimental situations and hope to report in detail on its empirical validity in the near future. I would, however, like to remark that a very interesting interpretation of this kind of two-stage model has recently been given by Restle [1963] in which he interprets the two stages of learning as the stage of conditioning and the stage of discrimination. Details of the model he proposes differs from that given here, but for most observable response patterns the differences between the two models will not be large.

Before turning to another topic I would like to emphasize that I do not feel that the analysis of concept formation in terms of the simple one and two-element models sketched here is fully satisfactory intellectually. It is apparent that these models must be regarded as schemas of the full process that is taking place in concept formation. What is surprising is that they are able to account for response data as well as they do. Theories which postulate more details about the learning process in concept formation are needed to go beyond the present analysis. This I take it will be particularly true as we proceed to the analysis of more complicated mathematical concepts, whose learning must rest upon the understanding of simpler concepts.

4. Logic and Mathematical Proofs.

Together with several younger associates I have been conducting pedagogical and psychological experiments with elementary school children on the learning of mathematical logic for several years. Before turning to a relatively systematic statement of some of our results, I would like to survey briefly what we have attempted.

In the fall of 1956 I brought into my college logic course a selected group of sixth, seventh and eighth graders (they were in fact no more selected than the Stanford students in the course). The demonstrated ability of these students to master the course and perform at a level only slightly below that of the college students was the initial impetus for further work. The next important step was the extensive study by Shirley Hill of the reasoning abilities of first, second and third graders. This study was begun in 1959 and completed as her dissertation in 1960. I shall report briefly on this below. In 1960 Shirley Hill and I wrote a text and taught a pilot group of fifth graders a year course in mathematical logic. This course was structured very similarly to a college logic course except that material was presented more explicitly and at a much slower pace. Students in the class were selected on the basis of their ability and interest (the minimum I.Q. was 110) and again the positive results were an impetus to further work. Because of the success of this class, the textbook was revised, and during the current academic year it is being taught to approximately 300 selected fifth graders in the Bay Area, with support of the project coming from the Office of Education and the National Science Foundation. These same classes will be given a second year as sixth graders, and in another

year we shall be able to report in detail on their level of achievement. We were also interested to see if we could train regular fifth grade teachers to teach the course as part of their regular curriculum. To this end we gave them a special course in logic in the summer of 1961 and all the classes are now being taught by regular teachers.

We began this past fall experimental psychological studies of how and to what degree children of still younger ages can learn the concepts of formal inference. I shall report briefly on a pilot study with first graders, which we are now in the process of redoing as a full-blown experimental study. On the basis of the experience of several of us with teaching logic to elementary school children we are now in the process of conducting an extensive psychological experiment with fourth-grade children to determine whether it is easier initially to learn rules of sentential inference when the standard interpretations are given or whether it is easier simply to learn the rules as part of an uninterpreted meaningless game. This last possibility will of course be most disturbing for a wide variety of mathematicians interested in the teaching of mathematics. I shall not enter here into the many reasons why I think there are good psychological arguments to believe that the initial teaching of inference simply as a game will turn out to be the most effective approach. I am frankly reluctant to formulate any very definite ideas about this highly controversial matter until we have accumulated a much more substantial body of evidence.

I turn now to the two experiments mentioned above which I want to report on briefly here.

Experiment VII. Logical Abilities of Young Children.

As already remarked, this extensive empirical study constituted Shirley Hill's doctoral dissertation [1961]. Dr. Hill gave a test instrument consisting of 100 items to 270 children in the age group six through eight years (first, second and third grades). Each of the 100 items consisted of two or three verbal premises plus a conclusion presented orally as a question. The subject was asked to affirm or deny the conclusion as presented. There were two primary reasons for not asking the children to compose a conclusion. In the first place, children of this age sometimes have difficulties in formulating sentences, and this has sometimes been cited as the reason for inappropriate measures being made of their reasoning abilities. The second reason is simply the methodological difficulty of interpreting the correctness or incorrectness of a conclusion given as a free response. The 100 items were equally divided between positive and negative answers. The first part of the test consisted of 60 items which were drawn from sentential logic. Every conclusion or its negation followed from the given premises by the sentential theory of inference. The second part consisted of forty items which were drawn from predicate logic, including thirteen classical syllogisms. However, the predicate logic items also included inferences using two-place predicates together with existential quantifiers.

Because it is easy for children to give the correct answer to a problem in which the conclusion is generally true or false, every attempt was made to construct the items in such a way that the omission of one premise would make it impossible to draw the correct conclusion.

To provide a behavioral check on this aspect of the items a base-line group of fifty subjects was given the test with the first premise of each item omitted. For instance, to quote the illustration given by Dr. Hill [1960, p. 43], the original item might read:

> If that boy is John's brother, than he is ten years old.
>
> That boy is not ten years old.
>
> Is he John's brother?

For the base-line group the item would be presented:

> If that boy is not ten years old, is he John's brother?

An example of a badly constructed item would be the following:

> If boys are stronger than girls, then boys can run faster than girls.
>
> Boys are stronger than girls.
>
> Can boys run faster than girls?

Naturally almost all children would give the correct answer to this latter item but their behavioral response would actually tell us little about their intuitive grasp of principles of logical inference. That Dr. Hill's items were well constructed are attested to by the fact that the base-line group got an average of 52.02% of the items correct, which does not significantly differ from chance. (Note that this percentage is based on 5,000 subject-items.)

I shall not go into all the facets of her study here. I mainly want to report on one or two of the most important conclusions. Let me first mention the results of the three standard groups of ages six, seven and eight years. The six-year old group receiving the items as described above got 71.18% of the items correct. The seven-year old

group got 79.54% of the items correct, and the eight-year old group got 85.58% correct. These percentage figures indicate a steady increase with age of the ability to draw correct logical inferences from hypothetical premises. In addition to the fact of increase, it is just as important to note that the six-year old children are performing at quite a high level, in contradiction to the view of Piaget and his followers that young children of this age are limited to concrete operations. Dr. Hill's study certainly provides substantial evidence to the contrary.

To avoid any possible confusion it should be borne in mind that it is not claimed that this study shows that young children are able explicitly to state formal principles of inference. What is claimed is that their grasp of the structure of ordinary language is sufficiently deep for them to be able to make use of standard principles of inference with considerable accuracy.

I would like to present just two other results of Dr. Hill's study. To avoid the conjecture that the children of age six may be able to do the simpler forms of inference quite well but do badly on the more difficult inferences involving two-place predicates, the percentage of correct responses for each age group on the ten types of inferences appearing in the 100-item test are shown in Table 4.

Insert Table 4 about here

The last two categories entitled "Quantificational Logic--Universal Quantifiers" and "Quantificational Logic--Existential Quantifiers"

Table 4

Percentage of Correct Responses for Different Principles of Inference by Age Level

Principles of Inference	Percentage of Correct Responses		
	Age 6	Age 7	Age 8
Modus ponendo ponens	78	89	92
Modus tollendo ponens	82	84	90
Modus tollendo tollens	74	79	84
Law of Hypothetical Syllogism	78	86	88
Hypothetical Syllogism and tollendo tollens	76	79	85
Tollendo tollens and tollendo ponens	65	77	81
Ponendo ponens and tollendo tollens	65	67	76
Classical Syllogism	66	75	86
Quantificational Logic-Universal quantifiers	69	81	84
Quantificational Logic-Existential quantifiers	64	79	88

refer to inferences that do not fall within the scheme of the classical syllogism. Although these last two categories are more difficult than the simplest modus ponendo ponens applications, the performance level of the children of six is still well above chance, and it is interesting to note that the performance on universal quantifiers is actually slightly better than the performance on sentential inferences using both ponendo ponens and tollendo ponens.

The second result concerns the attempt to identify some of the more obvious sources of difficulty. The lack of a sharply defined gradient in Table 4 suggested further examination of individual items. What turned out to be a major source of difficulty was the inclusion of an additional negation in an inference. Two hypothetical items that illustrate this difference are the following. Consider first as a case of modus ponendo ponens:

If this is Room 7, then it is a first-grade room.

This is Room 7.

Is it a first-grade room?

Let us now modify this example, still making it an application of modus ponendo ponens:

If this is not Room 8, then it is not a first-grade room.

This is not Room 8.

Is it a first-grade room?

The additional negations in the second item are a source of considerable difficulty to the children. It might be thought that the negations simply cause difficulty because they represent an increase in general complexity. To examine this question Dr. Hill compared the cases

using a single rule of inference in which negations occurred with the use of combined implications involving more than one rule of inference. The results are shown in Table 5. It is clear from this table that an

Insert Table 5 about here

additional negation adds a greater factor of difficulty than the use of more than one principle of inference.

Experiment VIII. Pilot Study on Mathematical Proofs.

The details of this pilot study are to be found in Suppes [1961]. The original study was conducted with the assistance of John M. Vickers, and we are now engaged in a larger study along the same lines. The primary objective of this pilot study was to determine if it is feasible to apply the one-element model described in Sec. 3 to the behavior of young children constructing proofs in the trivial mathematical system, described as follows: Any finite string of 1's is a well-formed formula of the system. The single axiom is the single symbol 1. The four rules of inference are:

$$\text{R1.} \quad S \rightarrow S11$$

$$\text{R2.} \quad S \rightarrow S00$$

$$\text{R3.} \quad S1 \rightarrow S$$

$$\text{R4.} \quad S0 \rightarrow S \quad ,$$

Table 5

Comparison of Increase in Error Associated with the Addition of Negation and with Compound Implications

Principles of Inference	Percentage of Error out of Total Possible Responses		
	Regular Form	Additional Negation	Combined Implication
Modus ponendo ponens	.06	.19	.17
Modus tollendo tollens	.12	.34	
Modus tollendo ponens	.03	.25	.27
Modus tollendo tollens	.12	.34	
Law of Hypothetical Syllogism	.08	.22	.16
Modus tollendo tollens	.12	.34	

where S is a non-empty string. A theorem of the system is, of course, either the axiom or a finite string that may be obtained from the axiom by a finite number of applications of the rules of inference. A general characterization of all theorems is immediate: Any finite string is a theorem if and only if it begins with 1 . A typical theorem in the system is the following one, which I have chosen because it uses all four rules of inference.

Theorem.	101
(1) 1	Axiom
(2) 100	R2
(3) 10	R4
(4) 1011	R1
(5) 101	R3

The proofs of minimal length in this system are easily found, and the correction procedure was always in terms of a proof of minimal length.

The stimulus discrimination facing the subject on each trial is simply described. He must compare the last line of proof in front of him with the theorem to be proved. This comparison immediately leads to a classification of each last line of proof into one of four categories: Additional 1's need to be added to master the theorem (R1): Additional 0's need to be added to master the theorem (R2): A 1 must be deleted to continue to master the theorem (R3); or a 0 must be deleted in order to master the theorem (R4). The rule in terms of whether the response should be made is shown in parenthesis. When the subject is completely conditioned to all four stimulus discriminations,

he will make a correct response corresponding to applying a rule that will produce a part of a proof of minimal length. For each of the four discriminations with respect to which he is not yet conditioned, there is a guessing probability p_i , i = 1,2,3 or 4 , that he will guess the correct rule and thus the probability $1 - p_i$ that he will guess incorrectly. In the analysis of data it was assumed that four independent one-element models were applied, one for each stimulus discrimination. (It is a minor but not serious complication to take account of two possible responses both being correct, i.e., leading to a minimal proof; e.g., in the proof of 1111 we may apply R1 twice and then R3, or R1, R3 and then R1 again.)

The pilot study was conducted with a group of first-grade children from an elementary school near Stanford University. There were eighteen subjects in all divided into two groups of nine each. One group received the procedure just described, including a correction procedure in terms of which a correct response was always shown at the end of the trial. The other group,which was a group using a discovery method of sorts, was not given a correction procedure on each trial, but at the end of each proof, subjects were shown a minimal proof, or in the event the subject constructed a minimal proof, told that the proof constructed was correct.

The following criterion rule was used. A subject, according to the criterion, had learned how to give minimal proofs in the system when four correct theorems were proved in succession, provided the subject had proved at least ten theorems. All subjects were given a maximum of seventeen theorems to prove, and all subjects, except for

two in the discovery group, satisfied this criterion by the time the seventeenth theorem was reached. The seventeen theorems were selected according to some relatively definite criteria of structural simplicity from the set of theorems which are of length greater than one and less than seven.

In Table 6 the mean proportion of errors prior to the last error in blocks of twelve trials for each group and for the two groups combined are summarized. A trial in this instance is defined as a step or line in the proof.

Insert Table 6 about here

More than sixty trials were necessary in order to prove the seventeen theorems, but because very few subjects needed the entire seventeen theorems to reach criterion the mean learning curves have been terminated at Trial 60. From this table it seems that the correction group is doing better than the discovery group, but I do not think the number of subjects or the total number of trials is adequate to draw any serious conclusions about comparison of the two methods. It is interesting to note that the discovery group has a much more stationary mean learning curve than does the correction group, and in that sense is satisfying the one-element model. Of course these curves are obtained by summing over errors on all four rules. It is very possibly the case that with a larger set of data for which it would be feasible to separate out the individual rules as the application of the one-element model described above would require, the correction group also would

have stationary mean learning curves for data prior to the last error on the basis of the individual rules.

In the near future we hope to be able to report on a much more substantial study that will give more definite information on these questions.

Table 6

Observed Proportion of Errors Prior to Last Error for the Correction, Discovery and Combined Groups. Blocks of 12 trials.

	Block				
Group	1	2	3	4	5
Correction	.28	.23	.15	.00	.10
Discovery	.23	.20	.40	.30	.33
Combined	.25	.21	.30	.18	.24

REFERENCES

Bower, G.H. "Application of a model to paired-associate learning," Psychometrika, vol. 26 (1961), pp. 255-280.

Bruner, J.S., Goodnow, J.J., and Austin, G.A. A Study of Thinking. New York: Wiley, 1956.

Burke, C.J., Estes, W.K. and Hellyer, S. "Rate of verbal conditioning in relation to stimulus variability," Journal of Experimental Psychology, vol. 48 (1954), pp. 153-161.

Coombs, C.H. "Psychological scaling without a unit of measurement," Psychological Review, vol. 57 (1950), pp. 145-158.

Estes, W.K. "The statistical approach to learning theory," in S. Koch (Ed.), Psychology: A Study of a Science, vol. 2, McGraw-Hill Book Company, Inc., 1959, pp. 380-491.

Hill, S. " A study of the logical abilities of children," Ph.D. dissertation, unpublished, Stanford University, 1961.

Hull, C.L. and Spence, K.W. "'Correction' vs. 'Non-correction' method of trial-and-error learning in rats," Journal of Comparative Psychology, vol. 25 (1938), pp. 127-145.

Murphy, J.V. and Miller, R.E. "The effect of spatial contiguity of cue and reward in the object-quality learning of Rhesus monkeys," Journal of Comparative Physiological Psychology, vol. 48 (1955), pp. 221-229.

Murphy, J.V. and Miller, R.E. "Spatial contiguity of cue, reward and response in discrimination learning by children," Journal of Experimental Psychology, vol. 58 (1959), pp. 485-489.

Restle, F. "Conditioning and discrimination: the all-or-none processes in paired associate learning," Technical Report, Contract Nonr 909(16), Department of Psychology, Indiana University, Bloomington, Indiana, 1962, 81 pp.

Stoll, E. "Geometric concept formation in kindergarten children," Ph.D. dissertation, unpublished, Stanford University, 1962.

Suppes, P. "Towards a behavioral foundation of mathematical proofs," Technical Report No. 44, Psychology Series, Institute for Mathematical Studies in the Social Sciences, Stanford University, January, 1962, 23 pp.

Suppes, P. and Atkinson, R.C. Markov Learning Models for Multiperson Interactions. Stanford University Press, 1960, p. 56.

Suppes, P. and Ginsberg, R. "Application of a stimulus sampling model to children's concept formation with and without an overt correction response," Journal of Experimental Psychology, vol. 63 (1962a), pp.330-336.

Suppes, P. and Ginsberg, R. "Experimental studies of mathematical concept formation in young children," Science Education, vol. 46 (1962b), pp. 230-240.

Suppes, P. and Ginsberg, R. "A fundamental property of all-or-none models, binomial distribution of responses prior to conditioning, with application to concept formation in children," Psychological Review, vol. 70 (1963), pp. 139-161.

Suppes, P. and Hill, S. Mathematical Logic for the Schools, vol. 1, 1962 edition.